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JUNE, 1866.

CIVIL ENGINEERING.

For the Journal of the Franklin Institute.

The Bourdon Pressure Gauge. By JOHN T. HAWKINS, U. S. N.

The rejoinder of the author of the original paper on the Bourdon Gauge, published in the *Journal* for April, attempts to show that the illustration given by me in my first reply was based upon a violation of certain settled principles of Mechanics. This attempt, it is evident from the arguments used throughout, is built upon, first, a misapprehension of the illustration; and, second, upon an assumption unwarranted in its application to this particular instrument.

He says: "The writer makes his grand *faux pas* when he assumes as established the following two remarkable points: 1. That in the same system of forces, when estimating their effects upon the same body, we are at liberty to take a different centre of reference for each force. 2. That the components of two forces will produce different effects from those of the original forces."

In the first place, it is not to be assumed that the line joining each pair of original forces is a rigid body, even though the diameter of the circular section of the tube be not increased; nor is the entire tube upon which the four original forces were supposed to act. If these forces produce motion within the body itself by their application at the points taken—whether we consider the line joining each pair of original forces or the entire tube the body—then the position taken was a correct one.

In my illustration the inner and outer boundaries of the tube were treated as separated bodies connected by the remaining material of the tube, that material being capable of extension, compression, and trans-

verse movement of its parts, as decided by the direction and intensity of the forces applied. If in the application of the original forces, or their components, the interior boundary suffered extension through its connexion with the exterior one, and the diameter of the circular section remained unaltered, the line joining each pair of original forces and the volume of the tube would both be increased; in which case, motion of the points of application must ensue, which is all that the "first principles of Mechanics" require.

That a curved tube without excess of internal pressure must, in order to preserve its volume, undergo a change from a circular to an elliptical section by being forcibly straightened, is but the converse of the fact that a similar tube with excess of internal pressure sufficient to preserve its circular form of section will, if forcibly straightened, be increased in volume, unless the forces applied tend to compress its outer boundary. It was the object of the illustration to show that the tube was thus straightened. Under these conditions a line joining each pair of original forces would no longer occupy the position of radius to either curve, and would, therefore, be increased in length.

In the second case, it was not attempted to show, nor can it be inferred from the illustration, that the components of two forces were to produce effects different from those of the originals, for the original forces may be shown to effect the same rotation about the two centres of reference, with the inner and outer boundaries considered as distinct bodies bound together by a substance capable of being distended and distorted transversely. The reason the assumed forces were resolved at all was simply to exhibit that, while they were not in a condition of equilibrium, so far as their effect in producing motion among the particles of the material of the tube was concerned, they were in reference to producing motion of the instrument as a whole. I therefore fail to see that it was so entirely unnecessary to thus resolve them, or that it in any way tended to confusion.

Again, in reference to my second figure, I did not "lose sight entirely of all intermediate arcs of metal." On the contrary, I not only held that in view, but, additionally, that the metal possessed a certain degree of lateral elasticity, which latter fact he seems to have entirely ignored, as if the material of which these tubes are composed was vulnerable only to transverse strains. He states that "this metal is not supposed to stretch between any of these points." I made no such supposition, nor can it be predicated upon either of my illustrations, except it be in a direction perpendicular to the axis of the tube.

I do not regard the case either of the copper steam pipe with short bends, the small pipes of the hydrostatic press, or the experiment with the rubber tubes and bulb, as anything of a "crucial test." In the first case, such pipes are always secured at both ends in such a manner as not only to prevent a separation of the ends, but also to resist any force exerted to rotate that portion of the pipe leading from the fixed end to the beginning of the curve about the fixed end as a centre, which must be done in addition to *producing curvature in the previously straight part of it.*

I certainly would not advance the idea that such tremendous effects

would be produced when I see the very moderate result of similar pressures in the Bourdon tube, with an elliptical section and one end free to move.

The second case is no different, for the pipes of a hydrostatic press are made very thick in order to sustain the enormous pressure, and this thickness is proportional to it. Its resistance to a change of form is, therefore, also proportional to the pressure.

In the experiment with the rubber tube and bulb, he admits that motion should ensue, if it were only what was due to an enlargement of its circular section, and yet there was no perceptible motion. This is a tacit admission, and, in fact, proves that the experiment was of too rude a nature to decide a question in which the motion resulting from the forces applied must be multiplied many times to render them readily observable, even under the most favorable conditions of the flattened Bourdon tube.

Whether the reasoning in my first reply was sound or not, I leave the reader to judge for himself. If, however, a "*faux pas*" has been committed, it is to be found in the charges made in the rejoinder referred to.

Deflexion of Beams.

From the London Builder, No. 1204.

Will you permit me to make a few observations with regard to the experiments on this subject recorded by Mr. Papworth in your number for the 17th inst.,* having paid some attention to the problem in question. Referring to the edition of 1845, page 81, of "Barlow," I find that the author says: "Hence we conclude that the deflexion of a beam fixed at one end in a wall and loaded at the other, is double that of a beam of twice the length, supported at both ends and loaded in the middle with a double weight." But Mr. Papworth is incorrect in calling this "the result given from numerous experiments made with perfect apparatus by Barlow;" the whole of the formulas being obtained by mathematical *reasoning*, and not as the result of *experiment*, which *reasoning* has been shown by all later investigators to be inacurate. Most of the formulas given by writers on the strength of materials are obtained by mathematical induction, and experiments are instituted in order to find the values of certain constants, (varying according to the *material* of which the beams are composed,) so as to make them applicable for practical purposes.

I would also call Mr. Papworth's attention to the table 73, pp. 110, 111, of "Barlow," in which experiments very similar to his own are recorded. The first set of beams are all "very dry," and have been "six years in store," (taking those only reported "sound.") Each is 2 inches square and projects 5 feet. The second set of specimens "were kept wet," and had also been "six years in store." The actual deflexions are not given, but on the previous page the mode of experimenting is explained, the beam being bent over a semi-circle of oak 6 inches radius. In the "very dry" beams the "arc received" is $3\frac{3}{8}$ inches average, and in those "kept wet" the average is $2\frac{1}{8}$ inches;

* See page 309.

so that the actual deflexion must have been at least twice as much in the former as in the latter. I refer to these experiments to show how necessary it is to take into consideration the *condition* of the timber at the time the experiment is made; and also that experiments on one or two beams are quite insufficient to settle the question.

The numerous experiments recorded by Barlow were made with a view of ascertaining the *breaking weight* of beams, so that the deflexions given are not such as can be calculated by for formulas; for, as Fenwick remarks, at page 82, "in practice only very small flexions are admitted," certain qualities being rejected in the investigation as being very small and unimportant when the deflexions are small, but which become appreciable when the deflexions are large, the real practical use of the formula being to ascertain what weight may be laid on a beam without producing perceptible deflexion. The only experiments given by Barlow which will allow of comparison with the formula are seen in table 70, page 106. These were made by Colonel Beaufoy, at Deptford Dockyard, on twenty-five pieces of Riga fir, all cut from the same tree, each piece being 2 inches square, and projecting 4 feet from the wall, and weighted at the outer end. The mean weight laid on was 98 lbs., and the mean of the deflexions of the twenty-five specimens was 1° 22′, "measured on a graduated arc of the same radius as the beam, viz: 4 feet." The vertical deflexion, therefore, is $48 \times \sin. 1° 22' = 1{\cdot}145$ inches. Now, the formula given by Fenwick, page 86, (omitting weight of beam,) is, deflexion $= \frac{4\ w\ l^3}{E\ b\ d^3}$, where b, d and l are all in inches, and E is the *modulus of elasticity*, say 2,000,000, the highest number for fir.

From this formula we get deflexion $= 1{\cdot}355$ inches, which is $\frac{2}{10}$-inch *more* than the experiments give, the timber being, no doubt, of first-rate quality. Barlow remarks: "It appears from all these experiments that the deflexions are very nearly in the ratio of the weights, till about one-half, or a little less than one-half the weight, is laid on, after which they become more rapid, and very irregular." So that experiment shows the formula would not be correct in this case if w exceeded 98 lbs.

In experiments of this sort great care is necessary to have the beam firmly wedged into the wall, as described by Barlow at page 114, as the least play will nullify the results. The formula which Mr. Papworth quotes from "Gregory" is identical with that given by "Fenwick," only in the former the length is in *feet*, and in the latter in *inches*, and I is the moment of inertia of the section; so that the formula applies to all forms of section, and in the rectangle $I = \frac{1}{12}\ b\ d^3$. In calculating the deflexion of a beam fixed at one end, and loaded with a weight, w, at the other, $\frac{3}{8}$ of the weight of the projecting beam should be added to w.

E. Wyndham Tarn, M. A.

Mr. Papworth's letter may render some service, if it lead practical men to make experiments on the deflexion of timber. Those whose

acquaintance with the calculus, and whose leisure may enable them to follow the elegant process by which deflexion (or rather the *law* of deflexion) is mathematically found, will not hesitate to affirm the correctness of the formulas given, and the conclusions drawn, by Mr. Tarn, (page 895 of your last volume,) quoting from Mr. Fenwick, conclusions no more peculiar to those gentlemen than the opinion that two and two make four. By *correctness* I mean, that if you grant the premises, you cannot escape the conclusion.

Of course, with a right formula we may have a wrong *modulus;* and here is a weak point. We want extensive, accurate, systemetic experimenting, to get at something like reliable average *moduli* of elasticity for different descriptions of timber. Not but that, when all is done, there will remain wide scope for observation and practical judgment in the endless varieties of elastic strength that occur even in one and the same kind of timber.

Again, the formulas go on the assumption that the piece of timber is *perfectly uniform* in size and elasticity throughout, with no tendency to bind irregularly or to twist.

With regard to the beam having one end fixed in a wall, the fixture must be *absolute*, and this is not an easy thing to ensure.

If a piece of timber, such as your last correspondent describes, (page 309,) were supported in the middle, and a ¾-cwt. hung at each end, 3 feet 6 inches from the support, and a mean taken of the deflexions where the weights hung, I think the experimenter would be in a better position to test the accuracy of the theorists.

To examine the first set of experiments reported by Mr. Papworth, and compare the results with theory, let us accept the fact that the piece of fir deflected $\frac{2}{3}\frac{1}{5}$, or 1·05 of an inch, with 1½ cwt. in the middle, and make this the basis of calculation.

Beam supported at both ends—

	Inches Deflexion.	
	Theory.	Actual.
With 1½ cwt. at quarter distance, . . .	0·59	0·60
With ¾-cwt. " "	0·30	0·35

Beam supported and fixed at one end—

With ¾-cwt. at D, ...	1·05	1·80
" " F, ...	0·44	1·08
" " E, ...	0·13	0·63
" " G, ...	0·02	0·25

The excess of deflexion, as observed, being 0·75 at D, 0·64 at F, 0·50 at E, and 0·23 at G. These excesses, being approximately as the distances from the support, suggest the idea of the support, or fixing down, having yielded or started, in some small degree, a thing not likely to occur were experiments made, as I suggest, with both ends loaded and the support in the middle.

If the observed depressions at G, E, F, and D were genuine *deflexions*, I trust your practical readers will bear with me when I submit that they would be as the cubes of distances of weight from support, or as

the numbers 64, 27, 8, 1. That such is far from the case, will appear on comparing the following figures:

Observed, ...	1·80	1·08	0·63	0·25
If as cubes of distances, would be	1·80	0·76	0·22	0·03

CHARLES E. CONDER.

Telemeters, or Instruments for Measuring Distances. By the late ARCHIBALD H. BELL, M.A.

Concluded from page 309.

From the London Mechanics' Magazine, November, 1865.

I now come to the consideration of those instruments which have been designed to meet the requirements of a particular case, viz: that of batteries in elevated positions overlooking the sea. It will be clear to every one that this is a special case, having the peculiar advantage that we are enabled to make use of the vertical height of the battery above the water-level as our base, instead of being limited to the few feet which can be provided by instruments, such as we have just been considering, designed for general purposes. These instruments are, first, the Gibralter instrument, proposed by Colonel Shuttleworth, R. A., in 1860, while in command of that station; second, the Malta instrument, erected in the Fort St. Elmo, the Castle of St. Angelo, and the Fort Figuè, at the time that General St. George was in command of Malta; third, an instrument invented by Captain Jerningham, R. N.; and lastly, an instrument of my own, which, for the sake of distinctiveness, I have called the hydroscope, and which, after a lengthened investigation, has been finally approved of and adopted into the service, it having been recommended by the Ordnance Select Committee "that a hydroscope be supplied to every battery, or group of adjacent batteries, of a height not less than 100 feet above the sea."

The Gibralter instrument consists of a telescope, to which a graduated arc is attached, provided with a movable index arm, carrying a spirit-level, and is mounted on a light tripod stand. The telescope being laid upon the object, the index arm is moved until the spirit-level shows it to be perfectly horizontal; the angle of depression is then read on the graduated arc, and the distance of the object found by reference to a table calculated for the known height of the battery.

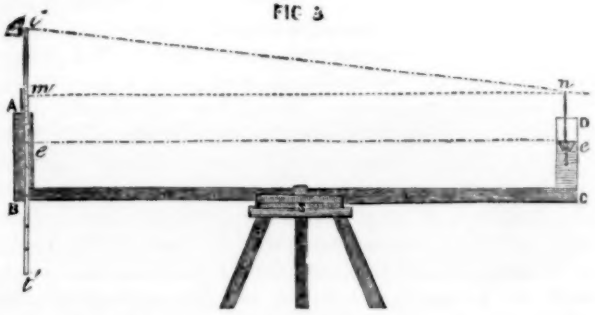
The Malta instrument is a massive semi-circular frame, of 3 feet radius, firmly fixed upon a masonry pedestal, and accurately leveled in all directions, in the centre of which works a vertical tangent scale, which is to be raised until the object at sea can just be seen over the rim of the semi-circle. The tangent scale may be either graduated with the angles, and reference be made from it to a table of distances, or the distances may be marked on the tangent scale itself.

Captain Jerningham's instrument consists of a plank of mahogany, 5 feet long, having a foresight at one end and a movable tangent scale at the other, and furnished at the centre with a spirit-level. Having leveled the plank first of all by little wedges, or whatever may be at

hand, the tangent scale is raised until the object at sea can just be seen over the foresight; the angle is then read on the tangent scale, and the distance determined by reference to a table calculated for the known height of the battery, which table, for convenience, is pasted upon the plank.

The following is a description of the hydroscope, which has been

FIG 3



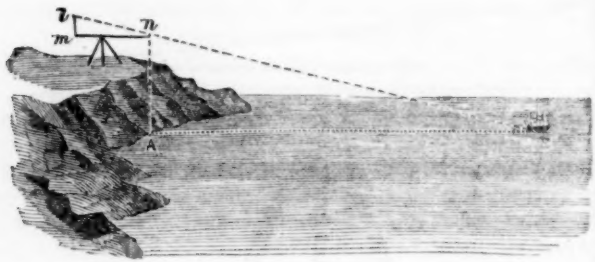
in constant use at Dover since August, 1862. It was made by a common tinman, at the cost of a few shillings, and it is only reasonable to expect that the instruments which are to be issued to the service will be in every way superior to the original, and obtain much better results than even those which have proved its practical value. (See Fig. 3.) B C is a piece of common galvanized iron gas-pipe, having attached to it, at each extremity, an upright rectangular tin tube A B and D C; the ends of the pipe at B and C are closed, but communication is open with the tubes A B and D C throughout from D and A. The pipe is clamped down by means of a small cap-square to a wooden bed S, which rests on a three-legged stool or on the top of a parapet. Into the instrument water is poured until the upright tubes A B and D C are about half filled. In each of these upright tubes there is a tin float, carrying, at right-angles to the line of the pipe B C, a small iron rod, having at the top a cross-bar of copper *m* and *n*. The floats are so weighted as to ensure the upper edges of the cross-bars being exactly level with each other when they float side by side in water. It will be evident, without further explanation, that, however the instrument be circled about upon its stand, the cross-bars *m* and *n* must always be perfectly horizontal. The tangent scale *t t'* slides up and down in a brass holder, which is confined in a groove of brass soldered on to the tube A, as shown in the engraving. This scale is of mahogany, square in section, with strips of sycamore inlaid on two of its sides, which are graduated to every 50 yards—one side to be used at high tide, the other at low tide.*

To use the instrument, traverse it round until it is in line with the object, then raise the tangent scale until the water-line of the object can just be seen over the top of the cross-bar *n*, through the cross-

* For other states of the tide, the mean between the two scales may be taken.

sight of the tangent scale. The distance is shown in hundreds of yards at the upper edge of the cross-bar *m*. Explanation of the theory is scarcely needed; the distance is to the height of the instrument above the water-level as the length of the instrument is to the length of the tangent scale. (See Fig. 4.)

FIG 4



Two instruments, one upon the Gibralter and the other upon the Malta plan, were made by Messrs. Elliott, of the Strand, to the order of the Ordnance Select Committee, at a cost of £7 each, and were sent to Dover for trial in the spring of 1861. The Malta instrument being required to be portable, was constructed of a stout semi-circle of cast iron, and furnished with a spirit-level. The two instruments were placed in a battery 233 feet above low tide, and observations were taken with both of them by different officers of the Royal Artillery quartered there at the time. In thirty observations taken with the Malta instrument, at distances varying from 300 yards to 1402 yards, the mean error was 0·050 of the whole distance, or 41 yards in the average distance of 834 yards. In the same number of observations made with the Gibralter instruments at the same objects, the mean error was 0·045 of the entire distance, or 37·53 yards. There was, therefore, but little difference between them in point of accuracy; but all the officers who used the instruments declared their preference for the Gibralter instrument, as being the quickest and easiest of management. It was reported, however, that it required an adjusting screw for the index arm; and the instrument got out of repair while at Dover. When it had been put in repair, and the proposed adjusting screw added, the two instruments were sent to Gibralter, where they were tested side by side in the Princess Caroline's battery, which is 557·808 feet above mean tide. They both gave good results up to 1893 yards, there being, as before, but little difference in point of accuracy; but the Gibralter instrument still received the preference, the Malta instrument being found "unsteady and difficult to level." The time occupied in making each of these observations was not given, and it is difficult to compare the accuracy of the results with any observations taken in England, as the height of the battery above the sea is so much greater than any in this country. Captain Jerningham's

instrument was tried at Plymouth and also at Dover, at heights varying from 230 feet to 300 feet. It was found to measure distances with sufficient accuracy, but the leveling of the plank was attended with great difficulty and loss of time, unless there happened to be in the battery some level pedestal for its especial use; and the moving up and down of the tangent scale was apt to shake the instrument out of its proper level. The plank was also found to bend and warp by very slight exposure to the weather.

The hydroscope has, as I have said, been in constant use at Dover since August, 1862; it has been repeatedly tested at stationary and at rapidly moving objects, and its efficiency has been practically exhibited with 40-pounder and 7-inch Armstrong guns at small targets up to 3300 yards, from batteries varying from 150 feet to 280 feet above the sea. It was favorably reported upon by a regimental board of officers at Dover in 1862, and by Colonel Shuttleworth, commanding the Royal Artillery in the western district, after trial at Devonport, in May, 1863, and has now been adopted into the service upon the recommendation of the Ordnance Select Committee. This instrument gives instantaneously the distance in yards of an object at sea, however rapidly it may be moving, from any battery not less than 100 feet above the water-level. Its perfect simplicity precludes the possibility of any mistake being made in its use, and it may be used by an uneducated person after five minutes instruction. From a battery 100 feet above the sea, distances up to 2500 yards can be accurately measured with it; from batteries of from 150 feet to 250 feet above the sea, such as Folkestone battery, and the Martello towers along Shorncliffe camp, we could measure 3000 yards; and from batteries of 300 feet above the sea, and upwards, such as there are at Dover, Plymouth, Gibralter, and many other of our fortications, distances up to 5000 yards may be accurately measured.

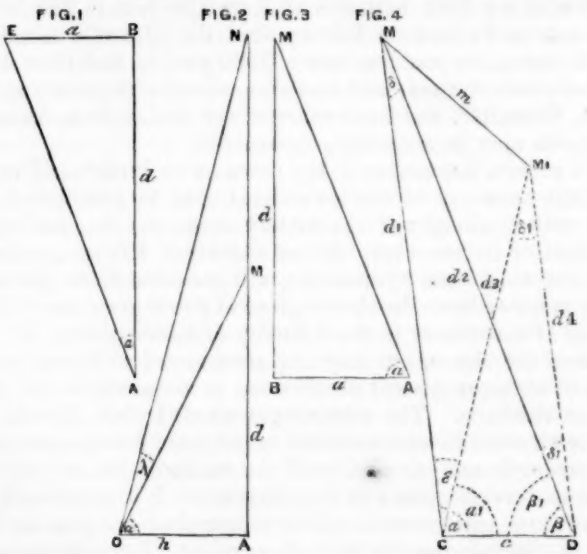
It is, of course, impossible to lay down as an established rule what the practical accuracy of the instrument is to be considered, as this will vary with the height of the battery above the sea, and also with the eye-sight of the observer. At an altitude of 150 feet, a good gunner, with the aid of the hydroscope, will measure 2500 yards within 30 yards, more or less. In observations at the lower ranges, the principal cause of inaccuracy is the difficulty of distinguishing the precise water-line of the object, but any one accustomed to laying guns will usually find his most distant observation to come within 50 yards of the correct distance. The advantages which I claim for the hydroscope, over all other inventions hitherto proposed for the same purpose, are, the strength and simplicity of the construction, its rapidity in operation, and its cheapness of manufacture. It is confidently anticipated that these instruments will be supplied at one-quarter the cost of either the Gibralter or the Malta instrument. The hydrocsope levels itself, and gives the required result without any reference to tables, or any calculations being necessary. It is thus the only instrument which can estimate at each moment the varying distance of a rapidly moving object. The instrument is not liable to accidental injury, and

errors cannot be produced by contraction, expansion, or bending of the tube. It is not liable to get out of adjustment, and errors of adjustment are easily recognized, and as easily rectified. On this point the Ordnance Select Committee report: "One of the great advantages of this instrument is its length of radius;* an error of 0·10 inch in either of the levels will represent an angular error of only 3·5 in the total depression, and this is a far larger error than is possible without great negligence, such as an observed leak in one of the floats, or its free movement being in some way checked."

Naval and Hydrographical Telemetry. By Dr. A. Gurlt.

From the London Mechanics' Magazine, December, 1865.

The following observations on the telemetric methods employed at sea, and a description of the new naval and hydrographical telemeter invented by M. Hermann Gurlt, engineer, Royal Prussian Navy, are intended as a supplement to a paper on telemeters by the late Mr. Archibald H. Bell, Royal Artillery, which appeared in the *Mechanics' Magazine* of November 3d and 10th. In that paper the various methods and instruments which have hitherto been in use on land for estimating distances are ably discussed. Amongst the latter the telemeters of Cavallo, Rochan, Professor Piazzi Smyth, Lieutenant-Colonel Clerk, Otto Struve, and the instruments invented for the use of coast batteries only by Colonel Shuttleworth, Captain Jerningham,



and the deceased author himself. As the methods and instruments therein described are, however, not applicable for naval purposes, the following remarks on methods of naval telemetry may prove accept-

* 100 inches has been selected as the distance between the floats.

able. Mr. Bell says with regard to these: "We require to estimate distances on board ship, *where not only the object, but also the observer, may be in motion.*" This is certainly the most complicated case of all, and as to instruments which will meet it, the author remarks justly: "This requirement is one of the navy rather than of our own profession, but it is of equal public importance, and though there are many difficulties in this form of the problem, there is every reason to believe its solution to be quite practicable and that it will be some day effected." This difficult problem has found a satisfactory solution already, through the invention of the instruments of M. Hermann Gurlt, and of which it is now proposed to give a description. Mr. Bell is not quite correct in remarking that all telemetric methods and instruments depend upon one fundamental principle, and upon which all our efforts to estimate distances must be built, viz: that, knowing the base of an isosceles or of a right-angled triangle, we can ascertain its height by measuring the vertical angle, as we shall see that scalene triangles may be used for this purpose as well, supposing that there are other elements known by which their values may be ascertained.

One of the commonest methods of estimating at sea the distance between two ships, is to measure with the sextant the angle a of the supposed right-angled triangle (Fig. 1) A B E, in which B E $= a$ represents the height of the other ship's mast, which is supposed to be known. The distance A B or d is then found after the formula $d = a$ cot. a by a very simple and easy calculation, for which purpose even tables may be prepared representing the value of d for the limited number of values a and a. This method appears to be very easy, but it is little commendable, as it admits of very considerable errors, and for the following reasons: The correctness depends, in the first instance, on the knowledge of the real height of the other ship's mast, which can only be guessed, and is consequently a source of great error. Besides, even if the real height were known, in case the ship rolls or is inclined leeward, the observer never knows how much of the real height he can see; and supposing the mast to be inclined leeward 39° out of the vertical line, the difference between the real and apparent height would produce an error as great as one-fourth of the distance. Besides the uncertainty of a, even the value of angle a is difficult to ascertain on board ship, as two points—say, the masthead and the part where it appears over the bulwark—must be simultaneously observed. Lastly, this method is not at all adapted for operations against coasts.

A second method is based upon the *exactly* known height of the observer in the rigging above the level of the sea. In Fig. 2, the distance A M or d of the other ship M, is ascertained by measuring from the elevated stand-point O, the angle λ which the two lines O M and O N form; the former being a line drawn from the observer's eye to the ship's water-line, the latter one to the horizon. As the angle A O N is known for the different values of A O $= h$, *i. e.* length of base-line of the triangle A M O, the angle A O M or a is ascertained by deducting λ or N O M from N O A, and the triangle supposed to be right-

angled, the distance d is found from the formula $d = h \tan. a$ for which tables might easily be calculated. Although this method is founded on a known instead of a guessed base-line, it is subject to several sources of errors, which affect its correctness considerably. It is evident that this correctness is much influenced by the correct observation of angle λ, which, however, is hardly possible, as the observer has to lay his instrument simultaneously upon two objects, and as this angle is necessarily small from the limited height of the base-line h. To this we must add that even the base-line is influenced by the oscillations of the vessel, and its value consequently much varied, and that it is impossible to lay any instrument exactly upon the water-line of a ship at sea, and much less so upon the horizon. From all this it follows that this method must needs be very incorrect, and as it is not applicable whenever the horizon is not visible, that is, when covered by coasts, thick weather, powder, smoke, &c., it is evident that it is very little use at all.

A third method is based upon the same formula as the one just described, but it has the advantage of a constant base-line A B $= a$, (Fig. 3,) fixed along the deck of the vessel. At B is a sighting instrument, so arranged that its axis forms a right-angle with the base-line; and at exactly the same moment when the object M, either by turning the ship or by any movement of the object M itself, passes before the sight, another observer, stationed at A, measures the angle α, and the distance $d = a \tan. \alpha$ is then easily ascertained. This, theoretically, very simple and—owing to its long permanent base-line—much favored method is, however, in practice hardly applicable, as for every observation the vessel requires to be brought exactly in such a position that her base-line forms the desired and supposed right-angled triangle with the lines A M and B M, which, however, is not very easy to accomplish. Besides, it labors under the disadvantage that the observation can only be made at a certain moment, and does not allow any repetition. From this it will be seen that the telemetric methods hitherto in use at sea are extremely imperfect, and that any method or instruments which will permit of accuracy in measuring distances at sea, both for purposes of naval gunnery and of hydrographical survey, are a real disideratum. This difficult problem appears at last to have found its solution by the method and instruments of M. Gurlt, so that little remains to be desired. M. Gurlt's new method rests—like all other military and naval telemetric methods—upon trigonometric principles; but as it is based on the measurement of any scalene triangles by means of a very ingenious instrument, its application is far more manifold and accurate than that of any other at present known, and as it can be used when either the observer or the object, or both, are in motion without any loss of accuracy, it must be admitted to fulfil even the most sanguine expectations. In the triangle C D M, (Fig. 4,) the two distances d_1 and d_2 are ascertained from the known base-line C D $= \beta$ and the two adjoining angles α and β after the formulas $d_1 = \frac{c \text{ sin. } c}{\text{sin. } \gamma}$

and $d_2 = \frac{c \sin. \alpha}{\sin. \gamma}$. The great superiority of this over all other methods consists in the exactly simultaneous observations of the two angles α and β, it being entirely indifferent whether they are right, obtuse, or acute, and in the use of a permanent, unchanging base-line. The method is simply carried into practice by measuring along the deck the constant base-line C D, and in placing at each end of it one of Gurlt's telemeters. The same object or part of an object, the distance of which from the base-line it is intended to ascertain, is then sighted with the telescope of each instrument, so that, following the motions of the object or those of the observer's vessel, it is always kept covered by the cross-hair centres of the instruments, when, at any given moment, both telescopes can be simultaneously fixed in their position through the action of two powerful electro-magnets, which are attached to each instrument and brought into play by a galvanic battery. The two observed angles α and β are then read, and they furnish the necessary elements for the calculation of the triangle sought. As this method must increase in accuracy with the length of the base-line, it is clear that C D should be made on board ship as long as possible, and as this length is practically unlimited on shore, it is evident that this method is of great value for the purposes of coast defence. Its very great accuracy, however, renders it most valuable for the hydrographical survey, which almost constantly has to solve the problem of the inaccessible distance, *i. e.* to find the distance from each other of two points M and M_1 (Fig. 4) which cannot be approached. It is only necessary to measure the triangles C M D and C M_1 D to find the angle $\delta = \alpha - \alpha_1$ and $\delta_1 = \beta_1 - \beta$; and, from two sides and the enclosed angle M M_1 or m, is ascertained in the triangle C M M_1,

$$m = \sqrt{d_1^2 + d_3^2 - 2\, d_1\, d_3 \cos.\, \delta},$$

and in triangle D M M_1,

$$m = \sqrt{d_2^2 + d_4^2 - 2 d_2\, d_4 \cos\, \delta_1}.$$

In order, however, to save the trouble of calculation, which would require some little time, and to comply with one principal requirement of a good telemeter, which Mr. Bell justly says is its capability of being used with rapidity, and showing the result at a glance, M. Gurlt has invented another auxiliary instrument, the "constructor," by which the measured triangle can be reconstructed *en miniature* in a few seconds and its values d_1 and d_2 read direct from a graduation.

The *telemeter* is, of course, and angle-measuring instrument, and its essential parts consist of an astronomical telescope, movable round a horizontal and vertical axis, and of a graduated semi-circular limb, upon which any angle between 0° and 180° may be read by means of a vernier and microscope, which are fixed to the telescope. The telescope rests with its cylindrical pivots upon a support, which is fastened to a circular base-plate, and can revolve with the same horizontally round a conical pivot. This pivot rests again in an universal joint fastened to a tripod. The base-plate bears two electro-magnets, which may swing slightly round a horizontal axis, and which are armed with

two pole pieces. Below this is another horizontal circular base-plate of a little larger diameter, which rests on the same conical pivot and bears two standards, in which the graduated semi-circular limb rests with its horizontal axis in such a manner that both the axis of this limb and that of the telescope coincide in one line, which is at right-angles with the axis of the conical pivot. In order to arrest the telescope it bears upon its pivots two vertical and semi-circular arcs of iron, and the lower bottom plate on its circumference a ring of iron; both act as armatures to the electro-magnets, one pole of which is attracted upwards by the vertical arc, and the other downwards by the horizontal ring, whenever an electric current is allowed to pass through the instruments. It is thus possible to fix both instruments simultaneously by magnetic attraction of four poles in each. The telescope bears a catcher by which it communicates to the graduated limb its vertical oscillations, whilst it slides along the same when turned sideways. The angles read upon it are formed by the base-line between the centres of both telescopes and their respective optical axes. The *constructor* is composed of two graduated arcs, round the centres of which two long arms can turn, each being provided with a vernier, micrometer, and microscope near the circumference of the arcs. The distance between the centres of these arcs represents *en miniature* the base-line on deck of the vessel, and by fixing the movable arms with screws upon the angles which were observed with the telemeters, they construct the measured triangle, and the values for d_1 and d_2 may be read from their graduation, where they intersect each other. The foregoing observations will doubtless contribute to the general knowledge and just appreciation of M. Gurlt's ingenious telemetric instruments, which will be found highly useful and accurate for all requirements of hydrographical survey, naval gunnery, and coast defence.

MECHANICS, PHYSICS, AND CHEMISTRY.

Considerations on the Loss of the "London." By WILLIAM FAIRBAIRN, C.E., F.R.S., &c.

From the London Journal of Science, April, 1866.

The introduction of iron as a material in constructive art has been attended with great advantages. For the purposes of ship-building it has given greatly increased strength, and afforded facilities for obtaining new forms, which, aided by the power of steam, have ensured a rate of speed in vessels never before attained in naval history. It has, moreover, furnished the naval architect with a material of immense value as regards construction, and its careful distribution in the shape of ribs, frames, and the sheathing of vessels cannot be too highly appreciated. As compared with the best English oak, it exhibits four times its powers of resistance, and it has, in addition, the double advantage of being almost perfectly homogeneous and free from the defects of open joints, which, in the case of the planking of wooden vessels, require to be caulked. With all these advantages,

iron constructions are surrounded with many dangers when entrusted to the care and superintendence of incompetent persons; in such hands there invariably exists a want of proportion in the formation of iron vessels, which exhibit defective powers of resistance, and such other abnormal conditions as might prove destructive to the efficiency and ultimate security of the structure. It is, therefore, necessary that the naval architect or builder should be conversant with the properties of the material employed, whether considered separately or in combination; and, moreover, he should be satisfied that the vessel, when finished, is capable of permanently resisting the forces of tension and compression, and all the varied strains to which she is subjected when afloat.

In laying down the lines of a ship, all these conditions should be carefully and deliberately considered. It is also of importance to take into account the forms or lines of least resistance, such as a fine entrance at the bows, and an equally clear run at the stern, if high speed is the object to be attained. In such cases, these forms are highly advantageous for vessels navigating rivers and smooth water, but in those intended for long sea voyages, and having to contend with the waves of the Atlantic, or the rolling seas of the Cape, it is questionable whether or not some slight sacrifices should be made to speed, and some modification effected in the form of the bows and stern, in order to meet all the requirements of a safe and convenient vessel intended for the double purpose of carrying passengers and cargo.

I have been led to these particular considerations, not so much from the lamentable accident which overtook the *London*, as from the conviction, that the safety and success of a vessel does not depend so much on its speed as upon its sea-going properties and sound construction. If, for example, we take one of the present iron clippers—which make such quick voyages—with her sharp bows and fine proportions, I am of opinion that she is neither the safest nor the best description of vessel to contend with a heavy sea in foul weather. In the first place, she is a *diver*, which cuts into the sea and rises with difficulty from a *bath*, which covers her decks with water as she pitches from sea to sea. But these are not the only objections to vessels of this form, as repeated immersions of this kind are exceedingly uncomfortable to those on board, and cause the ship to lift some tons of water before her buoyancy is restored to meet the next and every other succeeding wave into which she plunges in a rolling sea. It is not my intention from these observations to depreciate the value of speed either in the Royal or Mercantile navy. On the contrary, I thing it is the duty of every ship-builder to approximate as closely as possible to the lines of least resistance, which, in my opinion, ought to be carried to its utmost limits in smooth water, but in smooth water only.

In the construction of vessels of war, it was found expedient to rectify this want of displacement at the bows by projecting the submerged portion of the hull forward in the shape of a ram, not so much, however, for the purpose of attack, as to give buoyancy to the ship, and to enable her to rise more lively upon the sea. These defects of construction were observed in the iron-plated frigates *Warrior* and *Black*

Prince; the former vessel pitched and rolled heavily in the Bay of Biscay from similar causes, which from the first have been observable in all our high-speed ships. Viewing the subject in this light, it may not be out of place to suggest that all passenger and emigrant ships should be modified in their construction, so as to give increased displacement at the bows and stern, but more particularly at the bows, where they require buoyancy, having to encounter the force of a large body of water rushing over them and scouring the decks from stem to stern. Many of us remember the bluff, round bows of vessels of the last century, and how they rolled and pitched in a gale of wind. They were, however, short and compact, and although deficient in speed, they were, nevertheless, dry and excellent vessels at sea.

For several years I have endeavored to impress upon the minds of naval architects and others the necessity of increased strength on the upper decks of sea-going vessels, in order to balance the forces of tension and compression, and the double bottoms on the cellular principle of construction. The ultimate strength of a vessel is the resistance of its weakest part, and, this being the case, it is evident that it is of little or no value to have a strong double bottom if the deck is liable to be torn asunder by the alternate strains of a vessel pitching at sea. That these strains, often repeated, lead to fracture does not admit of doubt, and it has been proved by experiment that, under these circumstances, time is the only element in the endurance of the structure, and this varies according to the intensity with which the strains are produced. I offer these remarks from the conviction that heretofore the decks have been the weakest parts, and that several iron vessels have broken right in two from the constant working of alternate strains at midships along the line of the decks. I have, also, by way of illustration, compared an iron ship to a hollow girder, supported at each end and resting on the middle, for the exclusive purpose of showing the alternate changes to which she may be subjected if stranded or placed in the dangerous position of rising and falling on rocks in a heavy sea. Exceptions may be taken to these views, but they nevertheless exemplify what is necessary to be observed in the construction of a strong ship, and I may probably be excused the comparison, when the object in view is to effect security in the construction of our iron vessels.

I have been confirmed in my opinions on the forms and strain of vessels, from such facts as I was able to gather from the narrative of the loss of the ship *London*. From the accounts and the different statements of the witnesses examined before the Commissioners appointed by the Board of Trade, I was unable to discover any serious defect in the construction of the ship. On the contrary, I have reason to believe that both material and workmanship were perfectly sound, with the exception of the combings of the hatches, which it would appear were imperfectly secured. As respects the design, I have assumed that she inherited the extremely fine lines at the bow and stern already described, and to which I have directed attention, and additional weight is given to this opinion by the manner in which the vessel behaved at sea. Taking all the circumstances into account,

as also the statements of the different witnesses, with regard to the rigging and the state of the decks, I arrive at the conclusion that the ship did not founder from any serious defect of construction, excepting only the insecurity of the hatches, but from the hurried manner in which vessels are sent to sea, with their decks crowded with coal, hampers, and a variety of articles always dangerous and always objectionable in long and narrow vessels that are low in the water and liable to ship every succeeding sea. If these matters and the upper rigging had been properly cared for, there would have been no broken jib-boom to batter to pieces the combing of the hatchway, and instead of the *London* being entombed with all on board at the bottom of the Atlantic, she would, by this time, have been well advanced on her voyage to Australia.

For the Journal of the Franklin Institute.

Jets of Water.—Experimental data arranged, and some practical hints,
By S. W. Robinson, C. E.

The laws which govern the issuing of water from orifices of various forms depart so widely from mathematical theory, that it becomes necessary to adhere to the former in considering all questions relating to jets of water. These are only known from actual experiments, a large number of which have been made by numerous experimenters, mostly in Europe. The results of these experiments are certain numerical quantities, or co-efficients, each showing the deviation of the actual from the theoretical result for the particular case under consideration. In varying the conditions of the orifice, the fluctuation of the co-efficients is considerable, and is of great importance in all calculations pertaining to dynamic head, size and form of the orifice, quantity, the altitude and amplitude of the issuing jet.

It is sometimes desirable in practice to vary part of the conditions above-named, without changing others. This can only be accomplished by a thorough knowledge and judicious use of the experimental data, together with theoretical formulas. In this paper I propose to give some of the most important results of both theory and experiment upon jets of water, and show the use of both in a few examples.

The theoretic velocity with which water escapes from an aperture in a vessel, is the same as the acquired velocity of a body having fallen from a height equal to that of the water-level in the vessel above the orifice, which is—

$$v = \sqrt{2 g h}, \quad . \quad . \quad . \quad . \quad (1,)$$

in which h is the height, and g the force of gravity at the place.

If s equals the section of the orifice, the quantity, or flow of volume, called the efflux, or discharge, will be the product of the sectional area by the velocity, or

$$Q = s v = \sqrt{2 g h}, \text{ or} = s v t, \quad . \quad . \quad . \quad (2,)$$

for the time t, s being the base and $v t$ the length of the volume. This is the theoretic discharge.

The actual discharge is only secured by an application of the co-

efficient of discharge as established experimentally. Let m represent this co-efficient; then the actual discharge is—

$$Q^1 = m\,Q = m\,s\,v = m\,s\sqrt{2\,g\,h_0}, \quad . \quad . \quad (3.)$$

The co-efficient m varies slightly with the head and size of the orifice, and considerable with its form, and expresses the ratio of the actual to the theoretic discharge.

When the fluid is discharged through a thin plate, the vein undergoes a contraction or diminution of size, and a corresponding increase of velocity, the minimum section being at a distance from the orifice of about one-half times the mean diameter of the contracted vein, so-called, and the orifice. The ratio of the section of the contracted vein to the orifice is called the co-efficient of contraction; and the ratio of the velocity of the fluid particles at the contracted vein to the theoretical velocity is called the co-efficient of velocity. Let them be represented by n and u, respectively, and regarded as quantities belonging to the contracted vein alone.

Now, since the actual flow is the same at any section of the fluid vein, and equal the product of the section by the velocity, (2,) it follows that at the contracted vein—

$$u\,v\,.\,n\,s = \text{actual discharge} = m\,Q, \quad . \quad . \quad (4,)$$

in which $u\,v$ and $n\,s$ is the true velocity and section, respectively, of the contracted vein. But in equation (2), $Q = s\,v$, which substituted gives—

$$m = n\,u, \quad . \quad . \quad . \quad . \quad (5;)$$

that is, the co-efficient of discharge is always equal to the product of the co-efficients of contraction and velocity. At the contracted vein the velocity has been found to be very nearly the theoretic; consequently, the co-efficient of velocity is sensibly unity, and the co-efficient of discharge equal to the co-efficient of contraction. If a short cylindrical tube or ajutage, two or three times as long as its diameter, be applied to an orifice of the same diameter, the fluid will issue with but slight, if any, contraction; a case in which the co-efficient of discharge becomes very nearly equal to the co-efficient of velocity.

Among the most interesting experiments upon jets of water, and which give the greatest range in the values of m, n, and u, are those with conical tubes of various flare, the smaller base serving as the exit in one case, and the larger base in another. Further interest is added by prefixing an entrance mouth nearly of the form of the contracted vein.

I introduce here a table of co-efficients of discharge, velocity, and contraction, a table resulting from the labors of various experimenters, among whom may be mentioned Bossut, Mariotte, Castel, Venturi, Eytelwein, and others.

This table exhibits the co-efficients of discharge, velocity, and contraction, for ajutages differing in length from zero to 18·1 inches, and 50·d; and in flare from 50° convergence to 10¼° divergence.

TABLE *of co-efficients of discharge, velocity, and contraction.*

Head in feet.	AJUTAGE. Diameter, inches.	Length, inches.	Convergence.	CO-EFFICIENT OF— Discharge.	Velocity.	Contraction.
				m	u	n
*1 to 10	·61	1·57	50°	·842	·985	·854
"	·61	1·57	40°	·880	·979	·900
"	·79	1·97	30°	·900	·978	·920
"	·70	1·77	20°	·929	·970	·958
"	·61	1·57	16°	·939	·968	·970
"	·61	1·57	14°	·943	·964	·978
"	·79	1·97	13° 40′	·956	·964	·992
"	·61	1·57	13° 24′	·946	·962	·985
"	·61	1·57	12°	·944	·955	·988
"	·70	1·77	10°	·941	·950	·991
"	·61	1.57	8°	·935	·936	·999
"	·79	1·97	6°	·935	·932	1.000
"	·61	1·57	6°	·924	·924	1·000
"	·61	1·57	5°	·916	·915	1·001
"	·79	1·97	5°	·927	·925	1·002
"	·61	1·57	4°	·905	·904	1·002
"	·79	1·97	3°	·915	·909	1·006
"	·61	1·57	3°	·892	·890	1·002
"	·61	1·57	2°	·872	·870	1·002
"	·61	1·57	1° 30′	·863	·863	1·000
"	·61	1·57	0° 0′	·829	·830	·999
h	d	d †	"	·62	1·00	·62
h	d	2·5 d	"	·83	·83	1·—
"	"	10· d	"	·78	·78	"
"	"	20· d	"	·74	·74	"
"	"	30· d	"	·70	·70	"
"	"	40· d	"	·66	·66	"
"	"	50· d	"	·62	·62	"
26·	·27	00·	"	·69		
1·	·39	"	"	·65		
2·	1·00	"	"	·62	Sensibly unity.	Same as Disch'ge.
9·	2·00	"	"	·61		
12·	3·00	"	"	·61		
12·	7·00	"	"	·62		
2·9	1·33	4·3	— 3° 30′	·93	·65	
"	"	13·1	— 4° 38′	1·21 Jet irregular.	·37	
"	"	18·1	— 4° 38′	1·34 Same as Disch'ge.	·30 At orifice.	
"	"	12·0 ‡	— 5° 6′	1·46	·45	
"	"	6·9	— 5° 44′	1·02	·44	
"	"	10·4	—10° 16′	·91	—	

* Each result, as far as represented for the head of "1 to 10" ft., is a mean of five or six results from five or six heads, varying between one and ten feet.

† This tube was too short for the jet to fill; consequently, the co-efficients are same as for a thin wall or partition.

‡ The lengths here given are the lengths of the conical ajutages themselves. But a mouth-piece, nearly of the form of the contracted vein, was attached to each, forming an entrance mouth. This mouth-piece was found to increase the co-efficient of discharge in the ratio of 2 to 3, in the diverging or cylindrical tubes, less than one foot in length.

The co-efficients of discharge from the various forms of the orifice fluctuate from ·61 or ·62, for an orifice through a thin side, to 1·46 for the orifice provided with the mouth-piece and flaring ajutage.

The efflux resulting from the latter combination is more than double that from an aperture of equal diameter through a thin partition. Venturi concluded from his experiments upon diverging tubes and the mouth-piece, that a tube nine times as long as the diameter of the smaller base, and with a flare of 5° 6′, would give a discharge of nearly two and a half times that through a thin side, and nearly one and a half times as great as the theoretic. By means of the mouth-piece alone, Eytelwein raised the co-efficient of discharge from about ·62 to ·92, and with only the flaring tube recommended by Venturi, from about ·62 to 1·18, and with both in combination, from about ·62 to 1·55. According to these experiments, it will be safe to state, for a general approximation, that the mouth-piece increases the co-efficient of discharge in the ratio of 2 to 3, the flaring ajutage in the ratio of 3 to 5, and both combined in the ratio of 2 to 5.

Experiment also demonstrates that the application of the mouth-piece and flaring ajutage to conduit pipes of considerable lengths, say 300 times the diameter, does not appreciably affect the discharge.

The conical converging tubes, which serve in part as a mouth-piece, increase considerably the co-efficient of discharge as compared with that for a cylindrical tube, for all cases within the limits of the table, and have a maximum between 12° and 14°. The co-efficient of velocity increases with the convergence from ·83 for the cylinder toward unity; but never, in any case, does the co-efficient of the velocity of projection pass that limit, whatever be the form of the orifice.* In the diverging tubes it exceeds the theoretic velocity at the orifice considered; but at the exit it falls much below. The jets, however, from these tubes cannot be relied upon where great regularity is desired, though the experimenters make no mention of irregularity for so great a divergence as $3\frac{1}{2}°$.

The heights to which water will mount in jets nearly vertical, disregarding the resistance of the air, are proportional to the squares of the velocities of projection. In the formula $v^2 = 2gh_0$, h_0 is the head, and also the theoretic height of jet for the velocity v. But the true initial velocity, instead of being v, is vu; consequently, the height of jet is—

$$h = \frac{v^2 u^2}{2g} = h_0 u^2 \quad . \quad . \quad . \quad . \quad (6.)$$

Substituting first $u = 1$, then $u = ·65$, we get—

$$h_0 = \frac{v^2}{2g}, \text{ and } h = \frac{v^2 \cdot 42}{2g},$$

the difference of which is—

$$h_0 - h = \frac{v^2}{2g} - \frac{v^2}{2g} \cdot 42 = \frac{v^2}{2g}(1 - ·42) = ·58\, h_0,$$

* Unless, from some cause, globules of air become mixed with the water, whose elasticity in passing the exit gives to the water an impetus by which it may rise ever higher than the fountain head.

a difference of more than one-half. But as we can hardly expect to make use of a co-efficient of velocity as low as ·65, let us take it at ·82 or ·83, for a short cylindrical tube. Then we will have

$$h = \cdot 67\ h_0;$$

that is, *the height of a jet from a short cylindrical tube is only about two-thirds that of a jet from an orifice in a thin side.*

These heights, however, are never fully realized, in consequence of the resistance of the air, together with the interference of the falling particles with the rising jet, when it is quite vertical. For this total loss of height, the experiments of Mariotte and Bossut lead to the empirical formula—

$$h^1 = h - 0{\cdot}003\ h^2 \quad . \quad . \quad . \quad (7).$$

In very small jets, the co-efficient of h^2 is sometimes taken as high as 0·01. The greatest altitude possible in any case results from inclining the jet slightly from the vertical, to avoid the interruption caused by the descending drops.

When water spouts from an orifice in any other direction than vertical, its path described will be very nearly the arc of a parabola. The air causes a slight but almost imperceptible deviation therefrom. Experiments show that a sufficiently close approximation for practical purposes is secured by using about ·98 of the actual velocity of projection, and proceeding with the following formulas:

The equation of the curve described, regarded as a parabola taking the origin of co-ordinates at the orifice, is—

$$h = x \tan.\ \alpha - \frac{g\, x^2}{2\, v^2\, u^2 \cos.^2 \alpha} = x \tan.\ \alpha - \frac{x^2}{4\, u^2\, h_0 \cos.^2 \alpha} \quad . \quad (8),$$

in which h is the vertical ordinate, x the abscissa, and α the angle of elevation, or projection, reckoned from the horizon.

The altitude, or greatest elevation of the jet above the orifice, is given by the formula—

$$h_1 = \frac{v^2\, u^2}{2\, g} \sin.^2 \alpha = h_0\, u^2 \sin.^2 \alpha \quad . \quad . \quad (9).$$

The amplitude, or distance to the intersection of the jet with the horizon of the orifice, is—

$$A = \frac{v^2\, u^2}{g} \sin.\ 2\alpha = 2\, h_0\, u^2 \sin.\ 2\alpha \quad . \quad . \quad (10).$$

The angle of projection, in terms of the altitude and amplitude, is—

$$\tan.\ \alpha = \frac{4 h_1}{A} \quad . \quad . \quad . \quad . \quad . \quad . \quad (11).$$

Formulas (9) and (10) follow directly from (8) by making the necessary suppositions, and (11) is obtained from (9) and (10) by eliminating h_0.

In the balance of the article I propose to illustrate, in some degree, the application and practical utility of the preceding table and formulas, by a few examples and comments.

It is sometimes desirable to so dispose a series of orifices at the ex-

tremity of the same pipe-main, as to give a compound or sheaf jet, with a variegated combination of altitudes and amplitudes.

EXAMPLE 1.—Suppose we have an effective head of $h_0 = 12$ feet, to determine the height of a vertical jet issuing from an orifice in a thin wall, and also from a short tube.

First, for an orifice in a thin wall: $u =$ unity, and from (6) or (9), since $\alpha = 90°$, we have $h = h_0 u^2 = h_0 = 12$ feet, u being unity. Applying the correction of (7) for vertical jets, we get $h^1 = 12 - 0{\cdot}003 \times 12^2 = 11{\cdot}6$ feet, for the correct height.

Secondly, for the short tube: $u = {\cdot}83$, which, in (6), gives $h = h_0 {\cdot}83^2 = 12 \times {\cdot}69 = 8{\cdot}3$ feet, which becomes 8·1 feet, after correcting for resistance of air and interference of particles by formula (7).

EXAMPLE 2.—Given the altitude, $h_1 = 10$ feet, and amplitude, A: first = 15 feet, second = 25 feet. Required the angle and velocity of projection in each case, the correction for the resistance of the air, and the form of each orifice to give the required jets from a common head.

First, $\tan. \alpha = \frac{4\,h_1}{A} = \frac{40}{15} = 2{\cdot}666+$; hence $\alpha = 69° 26'$. From (9) we obtain velocity $v\,u = \frac{\sqrt{2\,g\,h_1}}{\sin. \alpha} = 27{\cdot}10$, by substitution and reduction. But, from a consideration of the resistance of the air, we will have, according to what has been said, $v\,u = {\cdot}98\,v'\,u$, or $v'\,u = \frac{v\,u}{{\cdot}98} = \frac{27{\cdot}10}{{\cdot}98} = 27{\cdot}66$, for the required velocity of projection.

Second, $\tan. \alpha = \frac{4\,h_1}{A} = \frac{40}{25} = 1{\cdot}60$; hence α 58° 00′. Also, similarly, as before, the velocity $v'\,u' = 29{\cdot}91$, and $v''\,u' = 30{\cdot}52$.—Now, since v', v'' are the theoretical velocities required, and assumed to be due to a common head, we have $v' = v''$, and may be eliminated; whence $u\,30{\cdot}52 = u'\,27{\cdot}66$, or $u = u'\,{\cdot}906$; that is, the ratio of the co-efficients of velocity must be ·906. If, then, we adopt the cylindrical tube for one orifice, the other will have a convergence of about 5°. Similarly, if for one orifice we adopt a tube of 50° convergence, the other must have a convergence of about 3°.

Substituting the values of u, u' for the first case, $v = 33{\cdot}32$ feet, and $h_0 = 17{\cdot}26$ feet, and for the latter $v = 30{\cdot}99$ feet, and $h_0 = 14{\cdot}93$ feet.

EXAMPLE 3.—Let it be required to construct a sheaf jet of nine orifices, three nearly vertical, and six much more inclined, for which we have a common head of 15 feet. The three central jets are each to have an altitude of 14 feet, and amplitude of 1 foot. The six remaining ones each to have an altitude of 9 feet, and amplitude of 9 feet. Two forms of orifices are necessary, of different co-efficients of velocity. Required the two forms and their angles of inclination. The following quantities appear in the computation:

	For one of the three jets.	For one of the six jets.
Altitude $= h =$	14·	9.
Amplitude $= A =$	1·	9·
Tangent $\alpha = \frac{4h}{A} =$	56·00	4·00
Angle of projection $= \alpha =$ . .	88° 58′	75° 58′
Theoretic velocity $= v =$ . .	31·07	31·07
Velocity of projection for non-interference of air $= v\,u =$ . .	30·66	25·31
Velocity of projection for resistance of air $= ·98\ v\,u =$ . . .	30·05	24·80
Co-efficient of velocity required $= u =$	·987	·815

The orifices having these co-efficients of velocity are, for the first, the conical tube of about 50° convergence; and, for the second, the cylindrical tube about 4·7 as long as its diameter. These tubes should be set with the inclination of their axes corresponding to the angles of projection, and at such a distance from each other as to allow the free ingress of the water.

Suppose the discharge of the individual jets to be equal, and the aggregate discharge of the sheaf jet to be about 7· cubic feet per minute. Then—

	For one of the three jets.	For one of the six jets.
Discharge per minute, ...	·768 cu. ft. for one orifice.	·768 cu. ft. for one orifice.
Discharge per second, . . .	·0128 cu. ft.	·0128 cu. ft.
Velocity, (theoretical,) . . .	31·07	31·07
Co-efficient of disch'ge for equal orifices,	·841	·815
Section of orifice, ...	·00049 feet.	·00051 feet.
Diameter of orifice, . . .	·300 inch.	·305 inch.

Service-pipes for fire-engines are usually from two and a half to four and a half feet in length, of the same inner diameter as the hose coupling, say two and a half inches at one end, and about one and a half or two inches at the other, where the ajutage or nozzle is attached. The nozzle is usually from five to eight inches in length, with the inner surface conical, of five to ten degrees convergence, terminated frequently with a cylindrical continuation, which is probably no improvement whatever. Referring to the table, we observe there is practically no contraction of the vein for so great a convergence as 10° or 12°; hence, for a less convergence than this, the cylindrical extension simply interferes with the free egress of the water. Experiment shows that the more convergent the ajutage, the more smooth and beautiful will be the stream. The table exhibits a greater discharge, at a higher velocity, for a convergence of about 14°, and a

still greater velocity, but with less discharge, for 25° convergence. But the greatest velocity of all is indicated for simply a perforated disk, in place of the service-pipe, upon the end of the hose. The heights being proportional to the square of the velocities, we will have

H (5°) to H (14°) to H (25°) to H (disk) as ·85 to ·93 to ·95 to 1·00,
and the discharge as ·92 to ·95 to ·92 to ·64.

These figures show that the greatest aggregate result, of both height and velocity, in trials of fire-engines, is secured with a nozzle of about 14° convergence on the inside surface, a greater convergence than is common. These conditions may vary slightly for the great head or pressure brought to bear upon the orifice in such trials. Notwithstanding the above figures show the greatest velocity and height for the perforated discoidal cap upon the hose extremity, still, remembering that the section of the stream in the latter case is only ·65, that for the tubes, on account of contraction, and the retardation being greater for small jets, other conditions being equal, it is probable that the greater velocity will no more than compensate for such loss.*

It is probably safe to infer from the preceding that the most favorable form of the nozzle, for the greatest aggregate result in trials of fire-engines, follows from the use of a nozzle of about 14° convergence.

On the Properties of Parkesine and its Application to the Arts and and Manufactures. By ALEX. PARKES, Esq., of Birmingham.

From the London Journal of the Society of Arts, No. 683.

Continued from page 271.

In reply to inquiries by Admiral Sir Edward Belcher, it was stated by Mr. Parkes that the solvent employed in this process was naphtha —either vegetable or mineral; that the present price of parkesine ranged from 1s. per pound upwards, according to quality; that its specific gravity was about equal to that of gutta percha; that no experiments had yet been made with regard to the resistance of this material to cannon.

Sir Edward Belcher thought it was likely to be very valuable for filling in of the intervals between the plates and the backing in iron ships of war instead of teak. He thought it would afford greater resistance to shot, and there would be no splinters. He wished to know whether it was inflammable.

Mr. Parkes replied that it could be made almost uninflammable; and, moreover, when used in contact with iron, it had no tendency to produce oxidation. With proper machinery a ton weight of the material would be produced in half an hour. As a varnish, (a specimen of which was exhibited,) it could readily be applied to the bottoms of iron ships to prevent corrosion from sea-water. Experiments were

* In one experiment, made in accordance with my suggestion, by F. B. Gilman of Springfield, Vermont, using a forcing pump driven by water power, the stream reached a horizontal distance of 100 feet for the ordinary nozzle. On application of the perforated discoidal cap, all other conditions remaining the same, the jet reached a horizontal distance of 110 feet.

being carried on with regard to its imperviousness to the attacks of marine insects, but the results had not yet been ascertained.

Mr. Owen Rowland said: Having been for some time engaged in making experiments upon this material, with a view to its application to telegraphic purposes, he thought it would be interesting to hear the results of those experiments. He had watched with great interest the progress of this invention for the last three years, and, like all great inventions, it had had a great deal to contend against, because it was very likely to displace many articles which were now very much in popular favor and use. He had kept several specimens of various materials (this one amongst the number) exposed to atmospheric influences, under different conditions, during the time he was engaged, on the part of the Board of Trade, in testing different insulating substances. He found that atmospheric exposure had not the least effect upon this material. It was not rendered less elastic, and its toughness was not diminished, nor was their the least approximation to decomposition. In that respect, he believed it would be most valuable from its non-oxidizing properties. In regard to its application to telegraphic purposes, he was sure they would all say they could not but wish to see a material introduced which would extend the great advantages which the telegraph was calculated to confer upon society. There was room for many materials for this purpose, and he was sure telegraphy had suffered immensely from the doubts existing in men's minds as to the sufficiency of the insulators now generally employed. The battle of the insulators in telegraphy had rivaled that of the gauges in the earlier days of railroads. For a considerable time past the efforts made to furnish a new insulating material had been very great; from day to day combinations were produced, which he had submitted to the test of the particular form of electrometer he had before him, (Peltier's,) which was unerring. The results of his tests of the substance now under consideration led him to think that in this material they would eventually have an excellent, cheap, and efficient insulator. On the paper placed on the board at the back of the chair he had arranged specimens of the material of different qualities, and he had tested the insulating powers of each specimen. The instrument employed in the testing was charged to a tension of 50, representing 512 cells of Daniell's battery. He placed this instrument in contact with the insulating material, and then watched the fall of the needle. If the needle fell so much in a minute with one article and fell more with another, then the former was the superior insulator. The results of these experiments were given in the table at the end of the paper. He did not agree with the plan of testing cables which had hitherto been pursued; he believed it had not been half severe enough. He had tested some hundreds of miles of the Atlantic cable, and he considered the test was not stringent enough. Sufficient attention had not been paid to the temperature and dryness of the atmosphere. In making his (Mr. Rowland's) experiments, the room in which the testing was to be carried on was kept at a temperature of 61° to 65°, and the hygrometric state of the atmosphere carefully

ascertained. The first attempt with material No. 9 (in the table) showed a leakage; that was, the needle of the instrument fell down 11·5° in 25 seconds; with No. 3 it fell down the same amount in 180 seconds; with No. 2 in 510 seconds; with No. 1 in 1080 seconds; with No. 8 in 509 seconds, and this was an excellent insulator; with No. 10 in 2046 seconds, and so on with the rest, while ebonite took 1050 seconds. Ebonite, which had been very much used, had been generally regarded as a very excellent article for these purposes, but he considered it had been surpassed by this invention. No proper machinery had as yet been constructed for the production of this article as it was intended to be ultimately manufactured. It required the utmost cleanliness and purity in its manipulation when intended for insulating purposes. Those were important considerations in all electrical matters, and, having witnessed the manufacture of several telegraphic cables, he was pleased to see that this material, even though as yet imperfectly manufactured, promised so well as an insulator. He remembered that in the first manufacture of gutta percha considerable impurities existed, and in many cases a really good insulating material had been condemned entirely through the foreign matters which it contained. He had a very strong opinion of the durability of this material. He saw no change in it, in whatever conditions he had placed it. He had boiled it in water, had exposed it to a hot sun, and had tested it in conditions to which it would never be exposed when used or electrical purposes, and he found little for no effect produced upon it. What was required for the perfecting of the invention was machinery into which no impurities could enter. He was very glad to find this invention brought before the Society of Arts, as had been done in the case of some of the greatest inventions ever produced. Twenty-one years ago he assisted Mr. Fothergill Cooke in exhibiting an electric telegraph in this room, which, on its first introduction to the world, had a great deal to contend against. He confidently expected to see this new material taking its place in the great and important work of extending electric telegraphy.

Mr. Wilson inquired the means by which this material could be joined together?

Mr. Parkes replied it was joined by means of its own solvent, and became a homogeneous mass.

Mr. Benjamin Fothergill asked whether, in the event of this material being employed on iron for rollers, there was any danger of its becoming disunited from the iron?

Mr. Parkes replied that, from its elastic and cohesive property and contractile force, he could not imagine that it would separate from the iron.

Mr. Taylor remarked that, as the inductive capacity of materials employed in telegraphy was a point of great importance, he should be glad to hear whether this material exhibited any advantages in that respect.

Mr. Rowland replied that the figures he had given might be said to represent the inductive capacity of the material. He believed it to be about equal to india rubber in that respect.

Dr. Bachhoffner said the variety of applications to which this material seemed to be adapted rendered it one of considerable interest and importance. As to its applicability to telegraphic purposes, practical trial alone could decide the question. The conclusions arrived at by Mr. Rowland, as the result of his experiments, were entitled to weight, but he believed it was a question whether that perfect insulation spoken of was the best condition for an electrical cable. However that might be, there could be no doubt that in this invention a very important addition had been made to our manufacturing resources, and he hoped his friend in the chair would not object to the inventor enjoying the benefits of a patent for it. It was always the privilege of an Englishman to grumble, and he claimed that privilege on the present occasion, inasmuch as having seen the announcement that "parkesine" was to be the subject of discussion this evening, he consulted every book in his possession in the endeavor to ascertain what parkesine was, but he failed in obtaining the information. He begged to raise his protest against inventions of this kind being called after the name of the inventor; for though, doubtless, this name would be handed down in connexion with the material, yet, as there were a great number of Parkes, many of that name might unjustly arrogate to themselves the merit of this invention. It was, however, often the case that great discoveries were associated with the patronymic of the inventor or discoverer, as in the cases of Galvani and Volta in connexion with galvanic or voltaic electricity, and MacAdam, the inventor of the modern system of roads; but he thought, from the multiplicity of purposes to which this material was adopted, it would be well to give it a name which would convey a better idea of what it was than could be gained by the public generally under its present title.

The Chairman said it was now his pleasing duty to ask the meeting to thank Mr. Parkes for his paper, practically illustrated as it had been by the observations of Mr. Rowland. He should, perhaps, have objected somewhat to this paper, in its present form, being read, had he not known that it would be supported, as it had been, as a matter of principle, by Mr. Rowland, whose views were entitled to so much weight. So far as his own individual opinion went, he thought it desirable that papers read before a Society like this should not have for their object merely the description of some particular invention, but should be more general in their character. No one, however, could question the importance of a discovery which introduced a material likely to be of great value in the arts and manufactures of the country, and the want of which was becoming more and more felt. We were exhausting the supplies of india rubber and gutta percha, the demand for which was unlimited, but the supply not so. In the case of gutta percha, the tree was destroyed in taking the produce of it, and we had to wait till other trees grew for future supply; and, with regard to india rubber, the plants only produced a limited quantity each year. This new commodity, however, was produced from materials of which there was an unlimited supply, and as such its applica-

tion to the purposes of electric telegraphy would be a matter of the highest interest and importance. Here he would make an observation in reply to the joke on the subject of patents, made by his friend, Dr. Bachhoffner, who was an advocate of one principle, while he (the chairman) supported the contrary view. That gentleman had taken the opportunity of assuming that he (the chairman) could not object to the inventor of this material having patented it, and deriving benefit from that patent. Certainly he did not object to his being benefited by the result of his labors through the means of a patent; for so long as the patent laws existed they were open to all; and he himself, though he thought, on the whole, they were injurious to the country, would not hesitate to benefit by them so long as these laws remained in force. But the argument of Dr. Bachhoffner was in reality in his favor. He objected to the name of parkesine being applied to the material, because there might be many "Parkes." That was, in fact, the great objection to the whole principle of the patent laws. In this country there was scarcely a process or invention, or a new application of science to the arts, that hundreds of persons were not engaged on at the same moment of time. It was only a question which would be first to go to the patent office and secure the benefits of that which was occupying the time of a hundred other persons, whose results would have appeared in due course if a patent had not been secured by one, who thus impeded the labors of the others. The result was to give a monopoly to one person in a matter which ought to be shared in by many. As the law now stood, it was not the man whose ability and scientific knowledge brought about a great invention who got the benefit of it, but it was the most active and energetic man of business, who, simply by being the first to secure the patent, derived all the advantages. It was clear, if they looked at the specimens on the table, that this material was applicable to a vast variety of uses, from a shoe-sole to articles of ornamental art, and he was sure the meeting would unanimously agree that Mr. Parkes was entitled to the thanks of the Society for the very interesting subject he had brought before them this evening.

The vote of thanks was then passed and duly acknowledged.

Scientific Aërostation.

From the London Mechanics' Magazine, July, 1865.

The art of traversing the air by means of balloons is of comparatively recent date. The germ of the invention of the balloon is to be found in the discovery by Cavendish, in 1766, of the remarkable lightness of hydrogen gas. It first occurred to Professor Black, of Edinburgh, that a light envelope filled with this gas would rise of itself, and he procured a thin animal membrane to make an experiment, which, however, he never carried out. Cavallo, in 1772, made the first practical attempt at ballooning by inflating swine's bladders with the gas. But he failed; and it was left for the Brothers Montgolfier to perfect the idea and to invent the balloon. The first attempt to

render the balloon of practical utility appears to have been made in 1794, during the wars of the Revolution, when an aërostatic institution was formed at Mendon, near Paris, for training a corps of "aërostiers" in order to observe the enemy by means of balloons. At the battle of Fleurus, near Charleroi, fought against the Austrians, a balloon, under the management of the corps, was present, and according to the testimony of Jourdan, the General in command, it rendered signal service. More recently, if report be correct, a similar method was adopted during the seven weeks' campaign in Italy, in 1859. Balloons have also been enlisted in behalf of science by a past generation as well as in recent times. On the 18th July, 1803, Robertson and Lhoest made the first ascent for avowedly scientific objects at Hamburg. These observers testified to the diminution of terrestrial magnetism, and the general feebleness of electrical and galvanic phenomena in the rarer portions of the atmosphere. These results were considered of such importance by the French Institute, that they arranged for another ascent. The management of the balloon and of the physical experiments was entrusted to MM. Biot and Gay-Lussac, who started on the 20th August, 1804. This experiment proved unsuccessful, and a second was undertaken shortly afterwards by Gay-Lussac alone, when he rose to a height of 23,000 feet. After a series of experiments, the French *savans* were unable to confirm the results given by Robertson and Lhoest; they could not find any perceptible diminution in the intensity of electrical phenomena in the upper air. Since that time until the present, very few scientific aërial voyages have been undertaken, the principal being that of Humboldt in America, those made by Mr. Rush, accompanied by Mr. Green as steersman, on behalf of the British Association during 1847–49, and those by MM. Barral and Bixio at Paris, in June and July, 1850. Although, from accidents, the two last-named ascents were of short duration, yet some interesting results accrued therefrom. Amongst other things, the aëronauts noticed that they passed through a cloud of icicles which appeared to them to sustain themselves in the air contrary to the laws of gravity. Upon their horizontal surfaces they saw beneath them an exact image of the sun, formed by the reflection of the luminous rays on the crystals of ice floating about in a foggy atmosphere. It was further observed that the temperature of the cloud was as low as minus 40°, a far greater degree of cold than has been experienced in later ascents. No facts of importance appear to have been established by any other ascents than the last noticed, and the contributions of balloons to positive science might be set down to almost nothing, until very recently, when the revelations made by Mr. Glaisher have given a fresh impetus to and awakened a new interest in aëronautics.

More than two years since, Mr. Glaisher communicated to the Royal Institution the results of eight ascents he had made for the purpose of scientific research in the higher regions of the atmosphere. Since that time he has made seventeen additional aërial voyages and has added many interesting and important facts to the annals of physical

science. In making a balloon ascent, a novel sensation is at first experienced; at great elevations, the air is exceedingly cold and dry, and should the aërial traveler rise to an elevation too great for his power of endurance he enters upon a painless death, which is only comparable to that of a mountain traveler who, benumbed and insensible to suffering, yields to the overpowering lethargy. As in the latter, so in the former case, the only means of safety lie in moral energy. The subjects of research by means of balloons may be classed under six heads. First. To determine the rate of decrease of temperature with increase of elevation, and to ascertain whether the results obtained by observations on mountain sides, viz: a lowering of the temperature of 1° for every increase of elevation of 300 feet, are correct or not. Second. To determine the distribution of the water in the invisible shape of vapor in the air below the clouds, in the clouds themselves, and above them, at different elevations. Third. To compare the results, as found by different instruments, together: 1. The temperature of the dew-point, as found by dry and wet thermometers, (free;) dry and wet thermometers, (aspirated, or air made to pass rapidly;) Daniell's dew-point; Regnault's dew-point, (blowing;) Regnault's dew-point, (air made to pass rapidly.) 2. To compare the readings of mercurial and aneroid barometers, &c. Fourth. Solar radiation, by taking readings of the blackened bulb thermometer fully exposed to the sun, with simultaneous observations of the dry bulb thermometer, and also of observations of Herschel's actinometer. In the fifth place, to determine whether the solar spectrum, when viewed from the earth and far above it, exhibits any difference; whether there are a greater or less number of dark lines crossing it, particularly at the approach of sunset. Lastly, to determine whether the horizontal intensity of the earth's magnetism is less or greater with elevation. Upon most of these points a large amount of information is afforded by Mr. Glaisher's experiments, the results of which he has recently placed before the Royal Institution in a lengthy and exhaustive communication.

In determining the first point, as to decrease of temperature, it was found that the state of the sky exercised a great influence, which rendered two sets of experiments necessary—one set with cloudy, and the other with clear skies. The results of a series of experiments show the decline of the temperature of the air with elevation, when the sky was cloudy, to be 4·5° at the height of 1000 feet, or 1° in 223 feet. From 1000 to 2000 feet the decline was 3·6°, or 1° in 278 feet; from 2000 to 3000 feet 3·7°, or 1° in 271 feet; from 3000 to 4000 feet 3·4°, or 1° in 295 feet; from 4000 to 5000 feet 3·3°, or 1° in 333 feet. From 9000 to 10,000 feet the decline was 2·2°, or 1° in 455 feet; from 15,000 to 16,000 feet 2·1°, or 1° in 477 feet; and from 22,000 to 23,000 feet 0·8°, or 1° in 1250 feet. Up to 5000 feet, the number of experiments upon which each result is based varies from 13 to 22. From 7000 to 16,000 feet the results are based upon four experiments, which were made on the 26th of June and the 29th of September, 1863, on which days the balloon was frequently enveloped in fog and clouds

to the height of three or four miles. The results above 16,000 feet were obtained on the former of the above days only, during the ascent and descent, the sky being covered with clouds when the balloon was between four and five miles high. The decline of temperature with elevation, when the sky was clear, was found to be 6·2° at 1000 feet, or 1° in 162 feet. From 1000 to 2000 feet 4·7°, or 1° in 213 feet; from 2000 to 3000 feet 3·8°, or 1° in 264 feet; 3000 to 4000 feet 3·3°, or 1° in 304 feet; 4000 to 5000 feet 2·9°, or 1° in 345 feet; from 9000 to 10,000 feet 2·4°, or 1° in 417 feet; from 14,000 to 15,000 feet 1·7°, or 1° in 588 feet; from 19,000 to 20,000 feet 1·3°, or 1° in 771 feet; and from 28,000 to 29,000 feet, the decline was 0·8°, or 1° in 1250 feet. Up to the height of 22,000 feet, the number of experiments varies from 7 to 17, and there can be but little doubt that the numbers showing the decrease of temperature are very nearly true, and approximate closely to the general law. Above 24,000 feet, the number of experiments is comparatively few, but the results accord with those obtained from experiments at lower elevations. Under a clear sky a decline of 1° in temperature takes place within 100 feet of the earth, and at heights exceeding 25,000 feet 1000 feet of vertical height have to be passed through for a decline of 1° of temperature. By adding together successively the decline of temperature for each 1000 feet, the whole decrease of temperature from the earth to the different elevations is found. The results with a cloudy sky are as follows: From 0 to 1000 feet the decrease was 4·5°, or 1° on the average of 223 feet; from 0 to 2000 feet 8·1°, or 1° on the average of 247 feet; from 0 to 3000 feet 11·8°, or 1° on 255 feet; from 0 to 4000 feet 15·2°, or 1° on 263 feet; from 0 to 5000 feet 18·5°, or 1° on 271 feet; from 0 to 10,000 feet 31·0°, or 1° on 321 feet; from 0 to 15,000 feet 42·1°, or 1° on 356 feet; and from 0 to 23,000 feet 51·7°, or 1° on the average of 445 feet. These results, showing the whole decrease of temperature of the air from the earth up to 23,000 feet, differ very considerably from those with a clear sky. In the latter case, the numbers showing the average increase of height for a decline of 1° of temperature from the ground, are all smaller than those with a cloudy sky at the same elevation. The experiments show that a change takes place in the first 1000 feet of 1° on an average in 162 feet, increasing to about 300 feet at 10,000 feet. In 1862, this space of 300 feet was at 14,000 feet high; and in 1863, at 12,000 feet; therefore the change of temperature has been less in 1863 than in 1862, and less in 1864 than in 1863. The experiments have all been taken at different times in the year. Without exception, the fall of 1° has always taken place in the smallest space when near the earth.

The observations upon the second head, for determining the degrees of humidity of the air, when treated in a similar manner to the preceding, show the law of moisture to be, with a cloudy sky, a slight increase from the height of 3000 feet. After this, a slight decrease occurs to 6000 feet, the degree of humidity being, at this elevation, nearly of the same value as on the ground. From 6000 to 7000 feet there is a large decrease, and then an almost uniform decrease to

11,000 feet. It increases again from 12,000 to 16,000 feet, and then decreases. With a clear sky, the law of moisture is shown to be a slight increase to 1000 feet, a considerable increase between 1000 and 2000 feet, a nearly constant degree of humidity from 2000 to 5000 feet, and subsequently a gradual decrease to 12,000 feet. The numbers at greater heights become rather irregular. Whether the sky was cloudy or clear, Mr. Glaisher invariably found that, at extreme elevations, the air became very dry, but, as far as his experiments went, never quite free from water. The results for temperature were laid down on a diagram at the end of 1863, and the resulting curve was a hyperbola. Continuing this curve upward and reading out the decrease of elevation, the result would be that, at the height of 50,000 feet, the decline of temperature from the earth would be 83°. At 100,000 feet or 19 miles, the decline would be 97°, at twice that distance 106°, at 100 miles 112¾°, and at 200 miles 116½°. This shows that large changes occur near the earth, amounting to 24° in the first mile, and becoming less and less the further removed, until the change from 100 to 200 miles is less than 3°. The foregoing results were deduced chiefly from experiments made in the summer and during the day. In order, therefore, to ascertain whether that law held good at all times of the year, experiments were made at other seasons. From these it was found that the season and the time of the day influenced the results very materially, the experiments made during the winter months differing very much from the general laws. The particulars of each ascent are such that they cannot all be combined or all used in deducing general laws. Recent ascents, made under similar conditions to those from which the laws of decrease of temperature were found, when combined do not affect the previous results, but ascents made in winter differ very greatly, and this deviation is of great importance. Mr. Glaisher met a strong current of air from the south-west nearly one mile in depth, which passed over this country on the 12th of January, and continued for many days, and which must have exercised great influence. This is the first instance of a stream of air of higher temperature than on the earth having been met with. The observation of this south-west current is highly important, as bearing upon the very high mean temperatures experienced during winter, the probability being that the variations of our winters are due to its fluctuations. The general result of the differences in the law of decrease of temperature, lead to the conclusion that the theoretical law of refraction now used must be abandoned, and that every observatory will have to determine its own laws independently.

With regard to solar radiation, the blackened bulb thermometer, and Herschel's actinometer observations, Mr. Glaisher found on the 31st of August last, that, at the heights of 7000 and 8000 feet, the blackened bulb thermometer, exposed to the full influence of the sun, read only 3° higher than the shaded thermometer. On the 29th of September, at the height of 14,000 feet, the excess of reading of the blackened bulb was 2½° only under a bright sun; the increase of readings of the actinometer was from 3 to 5 divisions only. At 13,000 feet

the excess of the blackened bulb readings increased to 4° and 5°, and the increase in one minute of the actinometer readings was 7 to 8 divisions. At the height of 3000 and 4000 feet the influence of the sun increased, raising the blackened bulb to 7° and 8°, in excess of the shaded thermometer; the scale readings of the actinometer increased to 20 and 25 divisions in one minute, and on reaching the ground the increase in the same time was from 45 to 50 divisions. On some occasions the readings of the shaded and exposed thermometers have been found to correspond, whilst in others, the blackened bulb thermometer has read lower than the shaded thermometer. Hence, it appears that the heat rays in their passage from the sun pass the small bulb of a thermometer, communicating little or no heat to it. Herschel's actinometer invariably gave similar results. The inference is, that the heat rays from the sun pass through space without loss, and become effective in proportion to the density of the atmosphere or the amount of water present through which they pass. Another point established by practical observation is, that the velocity of the air at the earth's surface is much less than at high elevations. On one occasion the balloon traversed a distance of 95 miles in 2 hours and 47 seconds, the horizontal movement of the air on the earth in the locality traversed being registered as 33 miles during the above time. On another occasion a trip of 35 miles was accomplished by the balloon in the same time that the air traveled 8 miles on the earth. Still greater was the difference in another instance, when the distance traversed by the balloon was 70 miles, and the distance traversed by the air below was recorded as 6 miles only. From a series of experiments the conclusion is, that at high elevations a considerable increase occurs in the velocity of the air, which increase, however, is not constant in relation to the velocity at the surface of the earth. Observations also determine the existence of widely different currents in the atmosphere, the wind, in some cases, having been found to change from east to west in the course of a voyage. Respecting the comparison of the temperature of the dew-point by different instruments, it is found that in all the experiments there is no certain difference in the determination of the dew-point by Daniell's and Regnault's hygrometers. This temperature, determined by the use of the dry and wet bulb thermometers, appears to be very closly approximate to the results obtained by either of those instruments. The balloon, as we find it, certainly claims a position among the most important of human inventions, and will continue do so if even it remains an isolated power. But this is not its probable fate; it has already done for science that which no other power has accomplished, and there are around us growing signs of its further utilization.

Non-explosive Gunpowder.

From the London Mechanics' Magazine, July, 1865.

We recently noticed the discovery, by Mr. Gale, an electrician of Plymouth, of a method whereby gunpowder may be rendered explosive

or non-explosive at will. Some interesting experiments, illustrative of this discovery, were made last Saturday in a large tent at the Wimbledon rifle gathering. The process, which is very simple, consists of mixing two parts of dark impalpable powder with the gunpowder to be rendered non-explosive, thoroughly incorporating the two, and the operation is complete. The mixture at first sight, differs little in appearance from the impalpable powder itself, except that on close inspection, the shape of some of the grains of gunpowder can be detected. Each grain of gunpowder is isolated by an envelope of the impalpable or non-explosive powder, and, as a consequence, when fire is applied to the mixture, the gunpowder will only explode grain by grain as the fire reaches it. A quick-match was thrust to the bottom of a bowl of the mixture and set on fire. After the match had burnt beneath the surface a few grains of gunpowder were exploded, one by one, until the match was consumed. To render the powder again explosive, all that is necessary is to place the mixture in a fine sieve and sift away the non-explosive powder. The separation apparently can be perfectly effected. No particles of the non-explosive powder adhere to the gunpowder, which burns after the operation as rapidly and with as little residuum as before. The inventor states that in five minutes a barrel of gunpowder can be rendered non-explosive, and in another five minutes be restored to its original combustible condition. A shell might burst in a store of this mixture in bulk without any explosion except that of the shell itself. Gunpowder so treated may be stowed with perfect safety in any common shed or warehouse, and stowed also in bulk. Another reason why great care requires to be exercised in storing gunpowder is, that it absorbs moisture from the air very readily, and if not kept thoroughly dry soon becomes unfit for use. The non-explosive powder is stated by Mr. Gale to be in this respect totally different from gunpowder; that is to say, it does not absorb moisture from the atmosphere, and, as a consequence, it protects the gunpowder with which it is mixed. The inventor burnt some gunpowder which he stated had been kept for months in the mixed form under an open shed. In spite of this exposure, the grains of gunpowder was as hard, and the combustion so rapid and complete, as if they had just come from the corning mill. It is true that the bulk and weight of the mixture are three times as great as that of the gunpowder itself, but it can be stored in bulk in very little, if any, greater space than is required by gunpowder in barrels; and, as regards the question of carriage, there is no doubt that, for commercial purposes, three tons of the non-explosive compound would not cost so much as one ton of gunpowder. Mr. Gale has been ordered by the Secretary of State for War to attend immediately before the Ordnance Select Committee, and submit his discovery to their investigation.

A New Explosive Substance.

From the London Mechanics' Magazine, August, 1865.

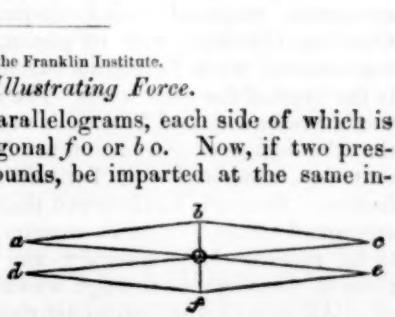
Glycerine, as we all know, is the sweet principle of oil, and is extensively used for purposes of the toilet, but it has now received an

application of rather an unexpected nature. *Galignani* states that in 1847, a pupil of M. Pelouze's, M. Sobrero, discovered that glycerine, when treated with nitric acid, was converted into a highly explosive substance, which he called nitro-glycerine. It is oily, heavier than water, soluble in alcohol and ether, and acts so powerfully on the nervous system, that a single drop, placed on the tip of the tongue, will cause a violent headache to last for several hours. This liquid seems to have been almost forgotten by chemists, and it is only now that Mr. Nobel, a Swedish engineer, has succeeded in applying it to a very important branch of his art, viz: blasting. From a paper addressed by him to the Academy of Sciences, we learn that the chief advantage which this substance, composed of one part of glycerine and three of nitric acid, possesses, is that it requires a much smaller hole or chamber than gunpowder does, the strength of the latter being scarcely one-tenth of the former. Hence the miner's work, which, according to the hardness of the rock, represents from five to twenty times the price of gunpowder used, is so short that the cost of blasting is often reduced by 50 per cent. The process is very easy. If the chamber of the mine present fissures it must first be lined with clay to make it water-tight. This done, the nitro-glycerine is poured in and water after it, which, being the lighter liquid, remains at the top. A slow-match, with a well-charged percussion cap at one end, is then introduced into the nitro-glycerine. The mine may then be sprung by lighting the match, there being no need of tamping. On 7th June last, three experiments were made with this new compound in the open part of the tin mines of Altenburg, in Saxony. In one of these, a chamber, 34 millimetres in diameter, was made perpendicularly in a dolomitic rock, 60 feet in length, and at a distance of 14 feet from its extremity, which was nearly vertical. At a depth of 8 feet, a vault filled with clay was found, in consequence of which the bottom of the hole was tamped, leaving a depth of 7 feet. One litre and a half of nitro-glycerine was then poured in; it occupied 5 feet. A match and stopper were then applied as stated, and the mine sprung. The effect was so enormous as to produce a fissure 50 feet in length, and another of 20 feet. The total effect has not yet been ascertained, because it will require several small blasts to break the blocks that have been partially detached by this.

For the Journal of the Franklin Institute.

Apparatus for Illustrating Force.

Let *a b c* o and *d f e* o be two parallelograms, each side of which is five times as long as the short diagonal *f* o or *b* o. Now, if two pressures, *d* o, *e* o, each equal to 5 pounds, be imparted at the same instant to the *rigid* body o, their joint effect upon it will be equal to that of a single pressure, *f* o, of 1 pound. This is easily proved by *experiment;* for if from an inelastic string, *a* o *c*, we suspend a weight of 1 pound, we *see*, by means



of spring balances at *a* and *c*, that there is a strain of 5 pounds along the direction of each line, *a* o, *c* o.

Again, if a moving force, *d* o, sufficient to move the body 5 feet, from o to *c*, in a given time, and a moving force, *e* o, sufficient to move the body 5 feet, from o to *a*, in the same time, be both imparted at the same instant to the rigid body, o, their joint effect upon it will be equal to that of a single moving force, *f* o.

Now, what I desire to know is, whether there is not some method of exhibiting to the *eye* the reverse of this last fact? What philosophical apparatus can I employ to show that the same amount of force which will move a body through a distance, and in the direction o *b*, of 1 foot, will, in the same time, move two bodies, each equal to one-half of the body o, through distances, in the directions o *a* and o *c*, each equal to 5 feet? I presume that apparatus for the elucidation of so important a principle must be in use in some of our institutions of learning, and a description of it would greatly oblige W.

Researches on the Vacuum. By HERMANN SPRENGEL, Ph. D.

From the Journal of the Chemical Society, January, 1865.

1. *The Instruments.*—The methods hitherto proposed for producing a vacuum may be divided into two classes, the mechanical and the chemical. A gas which fills a space may either be removed mechanically, or it may be converted, by taking advantage of its chemical or physical properties, into a non-gaseous, tensionless body. As, however, our atmosphere, from its general presence, is for the most part the only gas which has to be considered when a vacuum is to be formed, and as the reduction of its constituents to the non-gaseous form is attended with peculiar difficulties, I am inclined to consider that all practical methods of producing a vacuum may be regarded as mechanical. Atmospheric air may, in fact, be expelled from a space, first, by a solid body; secondly, by a liquid; and, thirdly, by a gas; which afterwards takes either a liquid or a solid form. Perfect vacua have hitherto been obtained only by this last method, but with such attending difficulties, that it appeared to me highly desirable to improve the other two, which afford greater facilities of working. The displacement of a gas by means of a solid or a liquid body is effected by instruments, commonly called air-pumps. The exhausting syringe of Otto von Guericke, with its piston, is the type of one class of these instruments, while Toricelli's barometer, with its column of mercury, is the type of the other class. The invention of the common air-pump was a consequence of Toricelli's vacuum, and it is partly due to this, that the degree of exhaustion attained by it has been, up to the present day, compared with Toricelli's vacuum as the standard of perfection. Hence it has followed that physicists have from time to time attempted to use Toricelli's vacuum as a receiver, or, in other words, to fill receivers with mercury, and attach tubes to them as long as those of barometers, through which the mercury was allowed to run off. All so-called mercurial air-pumps are based upon this principle,

and differ very little from the famous experiment which Toricelli made 220 years ago.

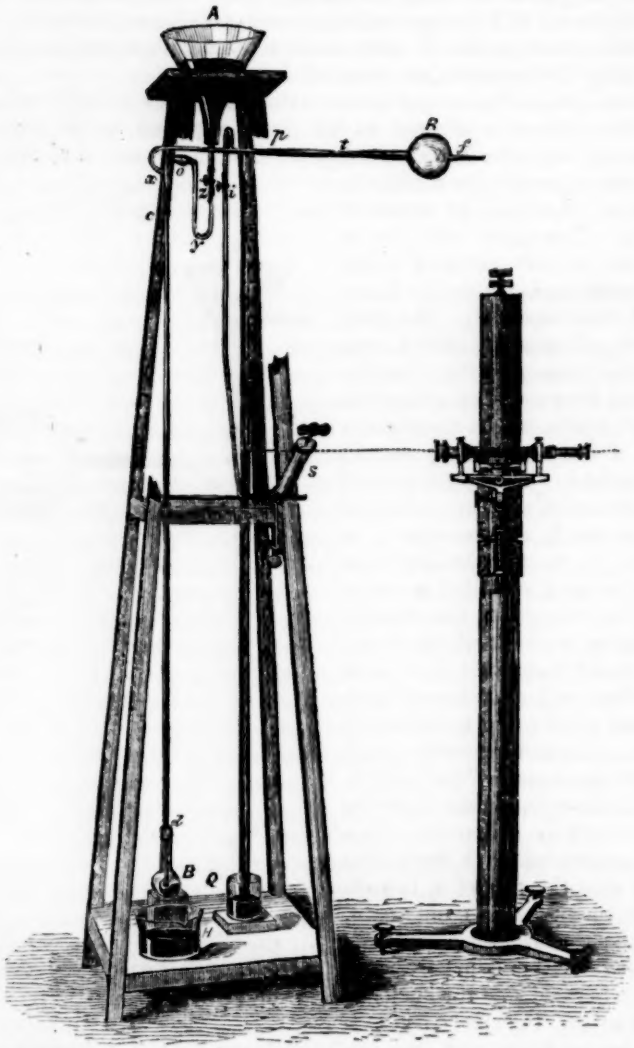
This, however, is not the only way in which the case may be viewed. If the top of a barometer were knocked off, the air would enter and the mercury would sink, or, what is the same, the mercury would sink and draw in the air. If, however, the experiment be so arranged as to allow air to enter, together with the mercury, and in such a manner that the supply of air shall be limited, while that of mercury is unlimited, the air will be carried away, and a vacuum produced. It is upon this principle that I have constructed a pneumatic machine, of which Fig. 1 represents the simplest form.

c d is a glass tube, longer than a barometer, open at both ends, and in which mercury is allowed to fall down, supplied by the funnel A, with which the tube is connected at *c*. The lower end *d* of this tube dips into a small glass bulb B, into which it is fixed by means of a cork. This glass bulb has a spout at its side, situated a few millimetres higher than the lower end of the tube *c d*. The first portions of mercury which run down will, consequently, close the tube, and form a safeguard against the air which might enter from below if the equilibrium should be disturbed. The upper part of *c d* branches off at *x* into a lateral tube, to which the receiver R is affixed. As soon as the stop-cock at *c* is opened, and the mercury allowed to run down, the exhaustion begins, and the whole length of the tube, from *x* to *d*, is seen to be filled with cylinders of mercury and air, having a downward motion. Air and mercury escape through the spout of the bulb B, which is above the basin H, where the mercury is collected. This has to be poured back from time to time into the funnel A, to pass through the tube again and again, until the exhaustion is completed. As the exhaustion is progressing, it will be noticed that the enclosed air betweeen the mercury cylinders becomes less and less, until the lower part of *c d* presents the aspect of a continuous column of mercury, about 30 inches high. Towards this stage of the operation, a considerable noise begins to be

Fig. 1.

heard, similar to that of a shaken water-hammer, and common to all liquids shaken in a vacuum. The operation may be considered completed, when the column of mercury does not enclose any air, and when a drop of mercury falls upon the top of this column without enclosing the slightest air-bubble. The height of this column now corresponds

Fig. 2.



exactly with the height of the column of mercury in the barometer, or, what is the same, it represents a barometer, whose Toricellian vacuum is the receiver R.

Fig. 2 represents the actual instrument with which I am in the habit of working. The funnel A, supported by a wooden stand, serves as the reservoir for the mercury. By means of the tube *z y x*, it is connected with the tube *c d*, which I call the "fall-tube," so that the mercury will run down, as soon as the clamp *z* is opened, which compresses a caoutchouc tube inserted there. The tube *x p* leads to the receiver which is to be exhausted, and is in connexion with two tubes, one of which is attached to the common exhausting syringe S, while the other, serving as a gauge, dips into a glass of mercury containing a barometer. When the instrument is at work, the rising of the mercury in this gauge will consequently show the degree of exhaustion. The exhausting syringe is merely attached as an auxiliary to accelerate the operation, because the fall-tube, for a reason to be presently mentioned, must be of a thin calibre. The greater portion of air is more quickly removed by the syringe, and after this has been done, as much as possible, and the connexion between the receiver and the syringe has been broken off by compressing an inserted caoutchouc tube with the clamp *i*, the remainder of the air is carried off by the running mercury. The bulb B and the basin H are exactly as in Fig. 1. The instrument is about 6 feet high, so that the mercury collected in the basin H can easily be poured back into the funnel A. The use of a pump would facilitate the raising of the mercury, and prevent the admixture of air. This latter inconvenience may, however, be pretty well overcome by gently pouring the mercury on a glass plate, floating on the surface of that in the funnel. Should a few air-bubbles attach themselves to the side of the funnel, it may be best to remove them by means of a wire or a glass rod, though they are not, perhaps, of much consequence, as they are not observed to pass along with the mercury. The connexions between the glass tubes are made of well-fitting black vulcanized caoutchouc tubing, sold under the name of "French tubing." This is free from metallic oxides, which render the tubing porous. Besides this, all these joints are bound with coils of copper wire, which is easily accomplished with a pair of pliers. Moreover, the space between the inside of the caoutchouc tubing and the outside of the glass tubing is filled with a resinous cement made of fused caoutchouc. To prevent this substance soiling the interior of the instrument, I first, after having put the instrument together, tie the caoutchouc joint with the copper wire, and then turn back the end of the caoutchouc tubing over the coil, coat the inside of the end with cement, and turn it back again into its proper position. From this it is obvious that the fused caoutchouc can only penetrate as far as the copper wire coil. The connexion of the funnel with the tube *z y o* is made by means of a perforated caoutchouc cork. (These corks are easily made from a flat block of caoutchouc, cut out with a sharp Mohr's cork borer, well lubricated with oil.) When the instrument has been put together in this manner, it is ready for use. At first, the mercury is allowed to enter the fall-tube in such quantities as to raise the mercury in the gauge as quickly as possible. When, however, the operation approaches its completion, which is shown by the

rattling noise, it will be found useful to lessen the supply of mercury, and to let it fall down, drop by drop, on the column of mercury in the lower part of the fall-tube, and to proceed in this way till the exhaustion is completed. I am not able to give any definite statement as to the quantity of mercury to be employed, as it is obvious that a small quantity, say an ounce or two, is capable of exhausting a receiver of an indefinite size, if this mercury is only made to pass the fall-tube often enough; but I may remark that I have found 10 to 15 lbs. of mercury a convenient quantity to work with.

In my endeavors to find out how to construct the instrument, in order to exhaust a receiver with the greatest economy of time and mercury, I have not met with satisfactory results. There, of course, exists a certain relation between the amount of air to be exhausted, the quantity of mercury to be employed, and the time of the operation. In order to make the instrument act at all, the supply of mercury must be at least so large that the fall-tube may become closed, *i. e.* the running mercury may form drops of a cylindrical shape, breaking off the communication between the receiver and the external air. As the supply of mercury is increased, the rapidity with which the air is carried off also increases. But this soon reaches its limit, as, should the mercury be admitted too rapidly into the fall-tube, it gains the preponderance and closes the aperture at x. The most favorable conditions under which the instrument might be used are those where the mercury is made to fall down, drop by drop, enclosing between every two drops as large a portion of air as possible.

Volume may be increased by extension either in height or in breadth. If the fall-tube be lengthened, the bulk of the enclosed air will be increased, and the time required to produce exhaustion will be shortened, without increasing the weight of mercury employed. But as it is inconvenient to have the instrument higher than the height of a man, I attempted to increase the second dimension, viz: the width of the tubes. I soon found, however, that it was difficult, if not impossible, to close tubes of more than a certain width by single drops. In order to close a wide tube with a cylinder of mercury, (or any other liquid,) the mercury (or this other liquid) must run in freely and not in drops, for the simple reason that drops cannot be formed of the diameter of the calibre of the tube. The size of the drops depends upon the specific attraction of the molecules of the liquid, the form and surface of the vessel from which the liquid drops, the attraction existing between the liquid and the vessel, and the resistance offered by the greater or less density of the air through which the drop falls. I have not been able to form in a vacuum drops of mercury larger than about 3 millimetres in diameter.

Having failed in the use of a wide fall-tube, I endeavored to effect my object by the use of several small fall-tubes. Here, however, another obstacle offered itself. It is exceedingly difficult to regulate the flow of mercury so evenly that exactly the same quantity shall run down in each separate fall-tube, and I have found in practice

that unless the flow of mercury can be so regulated, simultaneous action cannot be obtained in the fall-tubes.

From these experiments, I have found myself unable to produce a vacuum as quickly as with a common exhausting syringe, unless by the employment of inconveniently large quantities of mercury at a time. If speed is required, I think the fall-tube should have the addition of the exhausting syringe, which will take away more quickly the larger quantity of air and leave to the running mercury only the task of completing the exhaustion. By such a combination, however, the instrument loses much of its simplicity, and offers, by its numerous joints, a far greater chance of leakage. For this reason, where time is no object, it will be preferable even to do without the gauge and to use the instrument in its simplest form, as represented in Fig. 1. The operator will soon learn from observation of the way in which the drops fall down when the exhaustion is completed. Without the auxiliary air-pump, the exhaustion of a receiver of the capacity of about half a litre will take from 20 minutes to half an hour. Though this may appear to be a long time, I have no doubt this method will be found, after all, the quickest and simplest for producing a vacuum as nearly perfect as I have been able to produce.

The slowness of the action is obviously due to the smallness of the bore of the fall-tube. As soon as the calibre of this tube is increased, the time of the operation rapidly decreases; for the contents of two cylinders of the same height are to each other as the squares of their radii, and the time of the operation ought to decrease in the inverse ratio. The proper size of the bore of the fall-tube is $2\frac{1}{2}$ to $2\frac{3}{4}$ millimetres. As soon as I exceeded this limit, I invariably found the vacua less perfect. It is not difficult with these wider tubes to raise the mercury in the gauge to the height of the barometric pressure, minus 1 or even $\frac{1}{2}$ millimetre; but I have not been able to obtain with them vacua so near perfection, as I have been enabled to obtain by the use of fall-tubes of $2\frac{1}{2}$ millimetres calibre. The explanation of this fact must be sought for in the size of the drops, which, as it appears to me, must, in falling down, exercise a certain pressure against the side of the tube, thereby preventing the denser air underneath the drop from finding its way again to the part of the tube above the drop, where the air is more rarefied. I have not obtained better vacua by the use of fall-tubes of a calibre less than $2\frac{1}{2}$ millimetres.

Before I proceeded to test the efficiency of the instrument, I directed my whole attention to the construction of air-tight joints. In this, however, I did not succeed. It is a well-known fact that barometers become inaccurate in time, as air finds its way into the Toricellian vacuum between the glass and the mercury enclosed in it. The vacuum in my instrument is, of course, exposed to the same sources of imperfection. (To offer a greater resistance to the air, which might enter from the funnel, I have given to the tube *z y o* the form of a U-tube.) Leakage, however, happens in a far less degree from this cause than from the imperfection of the caoutchouc joints. Among the numerous

modifications I have tried, I consider the one which I have before described as the most practicable. With this joint the mercury in the gauge does not sink more than about $\frac{1}{2}$ millimetre in 24 hours. Williamson and Russel,* in constructing their admirable apparatus for the analysis of gases, have met with the same difficulty, and I am able to corroborate their statements. The porosity of solid bodies is astonishing, and one is almost compelled to think that glass vessels are the only ones impenetrable to gases.

The following degrees of exhaustion have been made with cold mercury, which had merely been filtered through paper to free it from visible impurities, but which had never been dried nor freed from air by special means. I have worked with heated mercury, (100 to 150° C.,) but have not noticed much difference in the perfection of the vacua. Even if some slight advantage could be obtained by it, its use would be objectionable, practically speaking, from the risk of endangering the health of the operator. When the mercury is heated and allowed to run down quickly, the instrument is at the same time converted into a sort of electric machine. In the dark flashes of electric discharge are seen to light up the exhausted tubes, and sparks may be drawn at intervals from the basin in which the mercury collects, as from an electrophorus. The fall-tube invariably becomes soiled after some time by some impurity in the mercury, and particularly after the employment of heated mercury. I attribute this to the oxidation of the mercury, arising from this electric action, which must be favorable to the formation of ozone. I have to mention that, to attain high vacua, the fall-tube must be clean as well as the mercury.

The length of the fall-tube in the instrument before us is calculated for the use of a liquid having the specific gravity of mercury. Of course, as the specific gravity of the liquid employed becomes less, the fall-tube must be longer. Practically, however, water is the only liquid that need be considered in place of mercury, and I have no doubt that an instrument adapted for water would furnish a simple and most efficient exhausting machine. It is not unlikely that such an instrument might possess advantages which air-pumps of other constructions have not, particularly in hilly countries, where the large volume of a natural water-fall might be rendered available.

I now come to consider the way in which this instrument acts. It is obvious that it stands in a near relation to the *trompe* or *Catalonian bellows*, the old and well-known contrivance for producing a blast. My instrument is merely the reverse of the trompe, with this addition, that the supply of air is limited, while that in the trompe is unlimited. The theory which explains the action of the trompe will, at the same time, explain the action of my instrument. The theory of the trompe has repeatedly been treated by distinguished philosophers, as Venturi, Magnus, Buff, and others. It would lead me too far to enter upon a criticism of their opinions, which appear to me partly erroneous, partly not to the point. In my opinion, the action of both instruments may, in all cases, be satisfactorily deduced from Kepler's law of the uni-

* Chem. Soc. J., [2,] ii., 238.

formly accelerated motion of bodies. When the clamp *z* is opened, only a certain and almost uniform quantity of mercury (or any other liquid) can pass at a given time. As soon as a particle of mercury has arrived at *x*, it is under the influence of the general law of gravitation. It must sink and move with uniformly accelerated velocity. The same may be said of the second or third particle of mercury; but while the second one is starting, the first one has accomplished a portion of its way, and when the third is starting, the distances between one and two and two and three are not equal, but unequal. A vacuum must, therefore, have been formed between them, and hence the tendency of the air to restore the disturbed equilibrium, *i. e.* by rushing in, if the instrument is open, or by expanding, if a receiver is attached. If the tube into which the liquid runs is larger than the column of liquid which the atmospheric pressure can support, the air in the receiver will, of course, expand to its last degree. If the mercury is allowed to fall down in *c d* in drops, it will act in exactly the same manner as the piston in a common air-pump. These drops are, so to speak, liquid pistons.

The chief excellence of this instrument appears to me to be its simplicity and the great perfection with which it performs its work. To ascertain the degree of exhaustion, I had at first to resort to a comparison with the barometer, but I have not been able to make a barometer in which the mercurial column stood higher than in the gauge of my instrument, though the barometers were constructed with care, and the readings made by means of a cathetometer. Though my instrument was not air-tight, and consequently not perfect, this apparent equality of the levels in the gauge and in the barometer is easily accounted for, upon reflection, by the fact that the human eye is not able to distinguish between $\frac{1}{1000}$, or even $\frac{1}{1000}$ part of a millimetre. But being curious to see how much the instrument, even in its present imperfect state, can do, I have taken particular pains to ascertain it. I have tried different ways, but I will describe only that which appears to be the best and most efficient. It is simply the application of Dumas' method for the determination of vapor densities. I took a receiver of the form R, (Fig. 2,) a bulb extended on both sides into a capillary tube, one of which was open and attached to the instrument, while a portion of the other was broken off and the aperture sealed. The part taken off, and having, consequently, the same calibre as the portion left attached to R, was preserved. The receiver was now exhausted and taken off by sealing it at A. This point was broken under the mercury which had just run through the instrument. I did this to meet the objection, that boiled mercury might absorb the remainder of the air in R, while, on the other hand, mercury containing more air might give off some of it and allow it to enter the vacuum. If, now, the receiver had been perfectly exhausted, the mercury would have filled it completely. This, however, was not the case, a very small air-bubble always remaining at the end of the sealed capillary tube. The capillary tube was broken off at *f*, and the mercury contained in R was collected and weighed. Into the capillary tube, first broken off

from the receiver, I now introduced by suction a small particle of mercury of exactly the same length as the particle of air in *f*. This mercury was then placed in a delicate balance and weighed. The weight of this particle of mercury bears the same proportion to the weight of mercury in R as the weight and volume of air remaining in R after exhaustion bears to the weight and volume of air in R before exhaustion. The highest proportion I have attained in this way is $\frac{1}{1300000}$, and upon the average I consider it not a difficult thing, with the present means, to exhaust a receiver to $\frac{1}{1000000}$. From this it is obvious that a barometer may be made by simply exhausting and sealing a tube, one end of which is then broken under mercury. I have actually made some in this way.

I have applied the finest reaction for the presence of gases, viz: the absence of any electric discharge in a perfect vacuum.* The few tubes I have made hitherto always showed a slight discharge, which, however, I have chiefly attributed to the presence of mercurial vapor, though I could have ascertained it by means of spectrum analysis. I have tried to remove this vapor by introducing between the instrument and the electrical tube, a tube exposed to a cold of —10° C., or filled with finely divided gold or freshly ignited charcoal, but the intensity of the whitish-green electric light appeared to be not much diminished by these means. From this I infer that the mercurial vapor has either not been condensed, or the supporter of light is due to another body. Being aware of the subtlety these experiments require, and of my shortcomings in their performance, I should not like to say more about them now, but I hope to repeat and study them with care hereafter. At any rate, I have learned this much, that during the exhaustion with my instrument, the colors of the electric discharge change from intense red and blue to faint white and green; that I have passed the limit at which the density of the air is most favorable to the electric discharge; that the stratifications are exhibited in an admirable manner, and that the instrument will prove useful for the performance and study of these experiments, which, on account of their beauty, have excited so large an interest.

Before concluding this paper, I should like to say one word more about the theory of the action of the instrument. Is the action entirely due to the accelerated velocity of the mercury and the elasticity of the air? I answer: yes. Struck by the extraordinary attenuation of the air, and misled by Venturi's theory, I was at first much inclined to attribute the action partly to two other agencies: the attraction of gases to liquids, and their absorption by liquids. But the following experiments showed me that I was mistaken: I forced water through short T-pieces at different velocities, using from a slight pressure up to that of several atmospheres, and I have not been able to raise water in a tube connected with the other branch of the T-piece to a greater height than that corresponding to the length of the tube from which the water was expelled. But this does not show so much as the fact, that when the calibre of the fall-tube is larger than 2½ millimetres, it

* Gassiot, Phil. Trans., 1858-59.

is impossible to raise the mercury in the gauge so high as it stands in the barometer, however long the mercury may be allowed to run, and whatever quantity may be used. This shows that the action is entirely mechanical; that the air expands and is cut off, portion by portion, by the falling drops of mercury, and that when the air is highly attenuated, these drops must entirely fill the tube, and even exercise a slight pressure against the sides of the fall-tube, otherwise the action will cease, as in a common air-pump, through the non-action of the valves.

I am under the impression that the use of better materials, and the application of greater skill than I have hitherto employed, will be followed by still better results, and will not improbably furnish instruments capable of producing vacua perfect to our senses; and even if we should not succeed in making a perfectly air-tight joint, the end will still be attained, if we can only succeed in carrying off the air more quickly than it can enter. At any rate, the immense elasticity of air is here displayed in a striking manner, and there is a very wide interval between the attenuation of 1,300,000, to the density of gases which must exist in the powder-chambers of cannons or mines at the moment of explosion. Another striking fact is that exhaustion of $\frac{1}{1300000}$ has been made with cold common mercury, doubtless containing a considerable quantity of air and moisture, which one would expect to be set free and enter the vacuum as soon as the mercury so violently agitated passes along the fall-tube. But the particles of these absorbed gases, which are set free on boiling, must, at common temperatures, be so intimately connected with the mercury that their expanding or gaseous properties are lost, as in the oxygen of oxide of mercury.

The main fact which I have established in this paper may be shortly stated to be that, *if a liquid be allowed to run down a tube, to the upper part of which a receiver is attached by means of a lateral tube, and if the height at which the receiver is attached be not less than that of the column of the liquid which can be supported by the atmospheric pressure, a vacuum will be formed in the receiver*, minus *the tension of the liquid employed.*

The properties of highly rarefied gases, and the conditions of that remarkable space in which there is *nothing*, have hitherto been scantily examined, though this subject is suggestive of interesting questions, the solution of which I hope to treat of in my further investigations that border on the, so to speak, negative side of natural philosophy.

The above experiments* were performed in the laboratory of St. Bartholomew's Hospital, and I am glad to have the opportunity of acknowledging most gratefully the facilities which have been offered to me by Dr. Odling in the prosecution of them.

* Communicated to the Chemical Society, June 16, 1864.

On some of the most Important Chemical Discoveries made within the last Two Years. By Dr. F. Crace Calvert, F.R.S., F.C.S.

(Continued from page 342.)

[From the London Journal of the Society of Arts, No. 673.

I shall now have the pleasure of calling your attention to an interesting and valuable invention of one of the most learned and eminent chemists of England, Dr. J. Stenhouse, F.R.S., who has devised quite a new method of water-proofing vegetable and animal tissues and fabrics. Previously to his discovery, the modes of water-proofing consisted in using beeswax and various kinds of drying oils, such as linseed, the siccation of which is enhanced by boiling them with peroxides of lead or manganese. Further, you are all aware of the extensive use which has been made of caoutchouc and gutta percha for water-proofing purposes. Dr. Stenhouse's water-proofing material is a white, solid substance, having no odor, undergoing no change through the action of the atmosphere, and which has acquired of late great popularity, by the application which has been made of it as an illuminating and lubricating agent—I mean paraffin, the discovery of which, in a commercial point of view, and its introduction into public notice, are due to Mr. James Young of Bathgate, near Glasgow, who has now established one of the largest manufactories in the world for the production of this article, notwithstanding it was considered a commercial novelty in 1852. Dr. Stenhouse found that if he employed pure paraffin for water-proofing, owing to its tendency to crystallize, it would not adhere sufficiently to fabrics. He therefore conceived the happy idea of adding to it a few per cent. of linseed oil, which overcame the defects presented when paraffin was employed alone, effecting a better adhesion between the water-proofing material and the textile fabrics, and rendering leathers more flexible. Dr. Stenhouse melts together paraffin oil with a few per cent. of linseed, as above stated. He runs the whole into cakes, and, in order to apply this water-proofing agent, he beats the cake and rubs the materials over with it, or spreads the melted mixture over the fabric by means of a brush. His process is applied with great advantage by Messrs. Silver & Co., to the water-proofing of soldiers' tents, and other materials of that class, to the great comfort of the soldiers, for, without increasing the weight of their tents, it renders them impermeable, and protects the men from rain and its attendant discomfort and danger. Another most useful application of Dr. Stenhouse's water-proofing material is the rendering of leather impermeable. By examining the specimens you will immediately see the immense advantage that cavalry will derive from having their saddles rubbed over with this preparation, as it renders the leather incapable of absorbing moisture, and enables the soldier to mount his horse after heavy rain with as much comfort as if it had remained under shelter. It also renders the soles of shoes quite impermeable, and at the same time communicates to them great flexibility, so that the boots of navvies, and other similar articles, are rendered far more useful and durable, as we all know that the constant

wetting and drying of leather expedites, in a marked manner, its decay. There is one more application of Dr. Stenhouse's water-proofing, to which I should wish to call your especial attention, as it is of interest to the manufacturers of Manchester and of Lancashire generally. In those districts large quantities of what is called water-proofing materials are used in packing the goods, and preserving them from external wet or injury. Many of these materials are made by covering a coarse calico fabric with a coating of boiled linseed oil; but this class of packing is very imperfect, and loses its strength rapidly, especially in hot climates, owing to the fact that the boiled oil absorbs oxygen, and carries it on to the fibre, oxidizing it, and thereby soon destroying its tenacity. By applying Dr. Stenhouse's process to the fabric, previously to the drying oil, not only is great impermeability attained, but the fibre, being saturated with paraffin, is preserved from the subsequent oxidation which it would undergo, under the influence of the atmosphere, in the presence of the boiled oil alone.

I should have wished to have dwelt at some length on the interesting application which has been made of late years, especially on the Continent, of another coal product, which, like most of those whose origin is due to coal, has received many valuable and beautiful applications. The one I now refer to is called bisulphide of carbon, and is applied with great success, not only to the extraction of fatty matters from various seeds and fruits, such as olive, linseed, &c., but in Algiers for extracting from flowers various essential oils, commonly called perfumes, such as essence of roses, lavendar, jasmine, &c. You will, no doubt, remember also the useful application which bisulphide of carbon receives in the hands of Messrs. Allbright & Co. of Birmingham, in separating the common phosphorus from the red or amorphous phosphorus now applied to the manufacture of chemical matches, as explained to you in my first lecture of last year's course.

I cannot conclude what I have to say respecting coal products, without stating the fact that, since I had the honor of delivering a lecture in this room, in 1862, many beautiful and valuable discoveries have been made in connexion with coal tar colors. Dr. A. W. Hoffman has succeeded in obtaining some magnificent purples, by a process as curious as it is highly scientific, and such as might be expected from so talented a chemist. Since then a splendid green color called verdine, and which has the curious property of retaining that color in artificial light, yea, more than that—of increasing in beauty in the presence of that light—has been discovered by M. Eusebe. A beautiful fast black, easily applicable to the art of calico printing, has also been devised; and, lastly, in the hands of Messrs. Simpson, Maule, and Nicholson, the well known color called magenta, and a beautiful blue called opal, have not only been much increased in beauty, but their cost of production has been materially reduced. In fact, at the present day, every shade of color is produced from coal tar products; but the subject is too vast for me to attempt now to enter into details as to their mode of production and application.

As the hour is advancing, I must, though with regret, leave the in-

teresting subjects which have reference to coal and its products, and pass to another class of manufacture. The first of these has reference to the refining of sugar, and among the several improvements which have been effected in this branch of manufacture during the last few years, none is more important in its general bearing than that recently brought to the notice of the public by Mr. Alfred Fryer, of the well-known firm of Fryer, Benson & Foster, of Manchester. This gentleman has published some valuable information on the composition of the sugar-cane juice, and the alterations it undergoes under the influence of heat and atmospheric air, and has conferred on the colonies a great boon by inventing an apparatus which will effect a great saving in obtaining sugars. Mr. Fryer's experiments on the cane juice teach us that, instead of only extracting 49 per cent. of the juice, as is usually the case, when the canes are worked by the old system, 61 per cent. can be obtained with a good steam engine having 24 inch rollers; and by pressing the megass a second time through the rollers as much as 70 per cent. can be obtained; and Mr. Fryer does not doubt that even 80 per cent. may be reached, if more attention is paid to the pressing of the canes through the rollers. Mr. Fryer has also published some interesting facts on the action of the oxygen of the atmosphere on cane juice. Thus he has observed that the juice rapidly darkens, and that this increase of coloration is not only an indication that the juice is undergoing deterioration, but that no amount of defecation will subsequently remove this coloration. He has further noticed that cane juice rapidly becomes acid, and the acidity increases also in a very rapid manner. He gives an instance—a pattern of too many others—where the juice, after expression, flowed down a spout 250 feet in length, from the mill to the boiling house, occupying in its descent about half a minute. The amount of acidity increased from 100 to 253, and the color deepened four-fold.

Mr. Fryer has also published some very interesting facts on the influence of heat on the cane sugar, and I here take the liberty of extracting from a paper of his the following facts:

"We now come to the boiling. All heat above 140° is capable of exerting an injurious effect. As regards the time, this effect is proportionate to the duration of the heat; the continuance of any syrup for two hours, at any given temperature, would cause just double the mischief which would be produced by its continuance at the same temperature for an hour. But as regards the heat, the mischief increases about as the square of the difference from 140° the highest innocuous temperature. So, at 160°, during any given time a certain degree of mischief would be done. To keep the same syrup for the same time at the heat of 180°, would give four times the mischief; at 200°, nine times; the difference of 180° from 140° being twice as great as that of 160°, and that of 200° being three times as great."

This mischief consists partly in the change of color, partly in the change of a quantity of cane sugar or sucrose into fructose. Both these kinds of sugar are present in all neutral saccharine solutions. Sucrose is the ordinary crystallized cane sugar; fructose exists more

especially in some kinds of fruit, such as grapes, gooseberries, cherries, oranges, &c., which refuse to form well-divined crystals. Further, I should remark that, under the influence of heat, as just stated, cane sugar or sucrose, is converted into grape sugar or fructose, and that the presence of fructose interferes in a most extraordinary manner by preventing the free crystallization of sucrose. Thus, for example, every particle of fructose in a mixed solution detains from crystallization its own weight of pure sugar. In fact, it would be impossible, after mixing equal weights of loaf sugar and fructose in a solution, to recover the former in a crystalline state. The change produced by the atmosphere alone, without the action of heat, shows the necessity of proceeding instantly to raise the temperature to the boiling point, and the concentration should be continued without loss of time. The temperature should not, however, be raised beyond the lowest effectual heat.

I will now call your attention to Mr. Fryer's "concretor," as he terms the apparatus. It consists of inclined corrugated plates, over which the juice flows.

From the further end of the cylinder a pasty mass is discharged, and, in cooling, it hardens into a non-crystalline homogeneous substance likely to be long known as Fryer's "concrete." Its material is simply cane juice deprived of its vegetable albumen and water, and, not being contaminated by molassas or caramel, is admirably fitted for the operations of the refiner, and will, therefore, command his attention. Allow me, further, to add that his process is easy to conduct; the apparatus, as you see, is simple and self-acting, not liable to derangement; that it performs a large amount of work, and therefore proves a material saving to the sugar producer. I cannot conclude my remarks on Mr. Fryer's invention, as applicable to the Colonies, better than by repeating here the words expressed by the Governor of Antigua, who said: "I believe firmly that you have opened a new era of prosperity to our Colonies, and heartily wish you abundant success in the course on which you have entered." As to the advantages which a sugar refiner in this country will derive from employing Mr. Fryer's "concrete," instead of having to refine the molassas and coarse sugars usually imported into this country, they are so obvious that it would be really a loss of time to enlarge upon them. The saving to the refiner in having nearly pure sugar to operate upon, and not having to contend with the removal of color, converted sugar, caramel, and other impurities, will at once convince you of the support and approbation the article will receive from the sugar refiner.

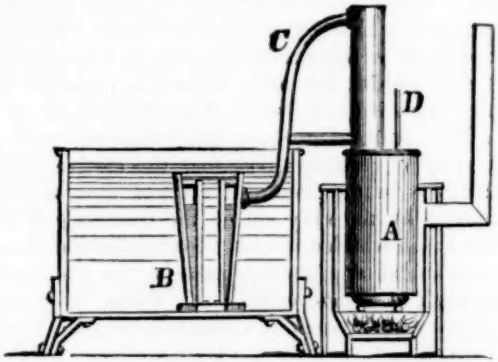
If, at the commencement of this lecture, I draw your attention to the value of intense heat produced at a small cost so as to enable our manufacturers to carry on their various processes, it now becomes my duty to inform you that of late years the cheap production and application of low temperatures has become a necessity, especially since organic products have been manufactured on an extensive scale. Three different machines for obtaining these low temperatures have

been brought to public notice, viz: First. That of Mr. Kirk, who has applied in a beautiful manner some of the physical laws discovered by Dr. J. P. Joule and Professor W. Thomson, in their researches on the mechanical theory of heat. Thus, Mr. Kirk succeeds in producing a low temperature by condensing under high pressure atmospheric air, which, on being allowed to resume its primitive volume, produces cold, for the compressed air cannot resume its primitive volume without the essential element for its expansion, namely, heat; and if the apparatus is properly contrived, which is the case in that of Mr. Kirk's, the heat necessary for the expansion of the compressed air is supplied to it by the body whose temperature is to be lowered. Although the construction of this machine is exceedingly costly, still it has been employed at Mr. James Young's works at Bathgate, with great success to the cooling of paraffin oil to extract from it the solid paraffin which it contains. The second apparatus is that of Messrs. Harrison & Co., of London, who employ ether as the medium for producing low temperatures. The third is that of M. Carré—in my opinion the cheapest and most practicable apparatus yet invented for the purpose, the more so that it is applicable for household as well as for manufacturing purposes. Although I, like many of you, have witnessed the production of ice by it at the late exhibition, still I was not prepared to learn that it could be applied with economy to the extraction of some of the salts existing in sea-water. Still, such is the fact. M. Ballard, a well known French chemist, after many years of study and labor, succeeded in extracting from sea-water two products, which play a most important part in most of our large chemical works, viz: sulphate of soda and chloride of potassium. To produce the first, under ordinary circumstances, extensive works are required, such as are found at St. Helen's, and Newcastle-upon-Tyne, where thousands of tons of common salt are acted on by vitriol, which gives rise to muriatic acid and sulphate of soda. As to the salts of potash, the French, as well as ourselves, are dependent for it upon the forests of Russia and the native forests of Canada. By M. Ballard's discovery, France will free itself from a dependence upon foreign supplies for the potashes she may require, as well as the sulphur, and will also do away with the public nuisance, namely, the conversion of common salt into sulphate of soda. Without entering here into details, let me state that M. Ballard operates as follows: In the early part of the spring season, considerable quantities of sea-water from the Mediterranean are run into large shallow reservoirs. In the summer season the water evaporates, a certain proportion of common salt separates, and the concentrated water is stored in other reservoirs until winter, when it is again allowed to flow back into the shallow reservoirs, where it yields, during a cold night, thousands of tons of sulphate of soda. The mother liquors from which the sulphate of soda has separated are allowed to flow into Carré's apparatus, where they are subjected to a comparatively intense cold, and yield large quantities of double chloride of magnesium and potassium, which, on being subjected to heat in a furnace, gives hydrochloric acid, magnesia, and

chloride of potassium. This application of Carré's apparatus, in this instance, shows the simplicity and practicability of it. It is based on rather a different principle to those of Kirk and Harrison. To obtain a low temperature he applies heat to a vessel which contains a saturated solution of ammoniacal gas. This drives off the gas, which is made to pass into a vessel surrounded with cold water, where it liquifies itself by its own pressure. If then the heat be removed from under the solution, its temperature will gradually fall, and it will become again susceptible of re-absorbing the ammoniacal gas, thus facilitating its evaporation from the vessel in which it has condensed, and, as it is necessary for the passage of the liquified ammoniacal gas into its gaseous form that it should absorb the heat from the surrounding medium, let it be water or any other fluids, their temperature will gradually decrease. The following diagram will enable you to understand better the working of this invention:

1. Before each operation incline the apparatus horizontally, and maintain it in the position about ten minutes. 2. Place the boiler (A) in the furnace, and the refrigerator (B) in a bucket filled with cold water, so that the summit of the refrigerator be covered with two or three inches of water. A small quantity of oil is poured into the tube which is placed in the upper part of the boiler, in which also a thermometer is placed. The apparatus is heated moderately until it reaches 266°. The apparatus is then removed from the fire, and the boiler is placed in the bucket of cold water. The hole of the refrigerator being stopped with a cork, and the tin vessel surrounded with alcohol, having previously nearly filled it with water, the congelation takes place by the evaporation of the liquid ammonia in the condenser.

Fig. 1.



Although the evening is far spent, I cannot leave you without calling your attention to a very ingenious mode, devised by M. Pelon, for warming railway carriages during cold weather. You are too well acquainted with the present clumsy and expensive method now in use to require any description from me. The only remark I shall make upon it is, that it is the boon of those who are in least need of it, namely, first-class passengers, whilst those of the second and third classes have not that luxury supplied to them. To avoid loss of time, I shall not attempt to describe the various methods which have been proposed as substitutes for the one at present in use, but shall at once state that

M. Pelon's method is based on the conversion of force into heat, the heat in this case being generated by friction; and from what I stated in my first lecture you must be perfectly *au courant* of what is understood in the present day of the conversion of heat into force, and *vice versa*. All those persons who were fortunate enough to visit the French Exhibition in 1855 must have been struck with a machine, invented by Messrs. Mayer and Beaumont, which generated enough heat to convert water into steam and thus obtain a motive power; and this was effected without fuel or electricity, but by mere frictional heat, which they obtained by causing a wooden mandril surrounded with tow to revolve with great rapidity inside a copper cylinder which itself was surrounded with a small quantity of water. The great merit of the invention of these gentlemen, and which drew upon them the special attention of the Emperor, was that neither the towed mandril nor the copper cylinder were materially injured or deteriorated after many days working. This important end was attained by them by allowing a gentle flow of oil to run constantly through the cylinder, thus preventing immediate contact between the towed mandril and the copper cylinder. In fact, the frictional heat was produced through the friction which took place between the particles of oil themselves, preventing thereby all wear and tear. Notwithstanding the ingenuity displayed in this invention, still, as a means of obtaining motive power, it was useless, for more force was required to generate an amount of heat than could be yielded by the friction of the particles of oil. But in the application which M. Pelon has made of this invention to the heating of railway carriages, the motive power required to generate the heat being produced without cost to railway companies, as it is caused by the very act of the traveling of the carriages themselves, the cause which accounted for the failure of the principle as a practical mechanical application, namely, the production of the force required to work it is overcome. M. Pelon proposes to fix one of Messrs. Mayer and Beaumont's mandrils under each railway carriage compartment, and to convey the heat produced by the revolution of the towed mandril to the compartment itself by means of metallic conducting surfaces, which, in their turn, will heat the atmosphere of the compartment. We shall, I am happy to say, within a year or two, know if M. Pelon's invention will be capable of general adaptation, as it is now being practically tested on one or two of the French railways.

Thin Sheet Iron.—Protection of Iron from Rust.

From the London Chemical News, No. 319.

We have, on previous occasions, reported the successive efforts of British and foreign iron-masters in the direction of producing sheets of iron of extreme tenuity. On the occasion of the meeting of the British Association, in September last, some large and very perfect sheets of iron foil were shown at the works of Messrs. Lloyds, Fosters

& Co. of Wednesbury, which weighed two grains per square inch, and we have already described the laminated metal produced by Mr. Parry of Ebbw Vale, weighing only 1·5 grains per inch. Since then great progress has been made by several manufacturers, and sheets of iron have been prepared of the marvellous degree of tenuity indicated by the measurement of 4800 thicknesses, being equivalent to one inch. They were produced in the mills of Messrs. W. Hallam & Co., Upper Forest Tinworks, near Swansea, and are in the form of perfect sheets, measuring 10 inches by 5½ inches, or 55 square inches, and weighing no more than 20 grains, or 0·36 grain per square inch. A noteworthy circumstance, in connexion with these thin sheets of iron, is their remarkable degree of permanence, or power of resisting the oxidizing action of the air. This protection is undoubtedly due to the continuity of the black fused layer of magnetic oxide with which these specimens are invariably coated, a fact which Mr. McHaffie of Glasgow has turned to useful account in a special furnace treatment, to which plates and other articles of wrought iron are subjected, for the purpose of securing increased protection against air and water. The operation consists merely in imbedding the iron plates in powdered hæmatite, or other native oxide of iron, and heating to full redness for several hours, when a perfect layer of the protective oxide is formed. The plates are then allowed to cool gradually, and are found to be especially well adapted to ship-building purposes. Mr. McHaffie, in his paper, also claims the use of oxide of zinc for producing an adherent black film, which, in this case, doubtless, consists of a true combination of the oxides of iron and zinc, possessing an equal, if not superior, degree of permanence to that which has been already mentioned.

Photographs on Paper in their Natural Colors.

From the London Journal of Science, April, 1866.

M. Poitevin has lately succeeded in producing photographs on paper in their natural colors. He prepares his sensitive paper in the following way: Having obtained a layer of violet subchloride of silver on the paper, by the action of light on the white chloride in the presence of a reducing agent, he applies to the surface of the paper a liquid composed of one volume of a saturated solution of bichromate of potash, one volume of saturated solution of sulphate of copper, and one volume of a solution containing five per cent. of chloride of potassium. This paper is dried and kept in the dark; it will keep good for several days. In this mixture the bichromate of potash is the principal agent, the sulphate of copper facilitates the action, and the chloride of potassium preserves the whites which are formed. In copying paintings on glass, the exposure to direct light need only last five or six minutes; but the time must, to some extent, depend on the transparency of the picture to be copied, and it is easy to watch the development of the image on the paper. The paper is not sufficiently sensitive for use in the camera. To preserve the pictures it is only necessary, first, to wash them with water acidulated with chromic acid,

then to treat them with water containing bichloride of mercury, afterwards with a solution of nitrate of lead, and, lastly, well wash them with water. After that they will not change in ordinary light, but will, however, turn brown in direct sunlight.

Magic Photography.

From the London British Journal of Photography, No. 312.

Such is the designation of the latest novelty in our art-science—a designation which we borrow from a sensational advertisement in one of the daily papers. Its nature may thus be stated: Two sheets of paper are supplied to the purchaser, together with instructions. One of these sheets is albumenized, the other is a sheet of blotting-paper. There is no picture visible on the albumenized paper; but when, in accordance with the instructions given, the sheet of blotting-paper is moistened by means of a few drops of water and pressed in contact with the face of the albumenized paper, a picture immediately springs into existence. The question now arises: How is this accomplished? Light has evidently nothing to do with it, seeing that the same phenomenon occurs both in sunshine and in comparative darkness.

The following is the method by which these "magic photographs" are produced: Print a picture on albumenized paper in the usual way, taking care not to print so deeply as ordinarily. Fix the print (without toning) in plain hyposulphite of soda, wash thoroughly, and then immerse it in a saturated solution of bichloride of mercury till the image disappears. Again wash thoroughly and dry. The paper now appears like a piece of plain albumenized paper, without any appearance of a picture on it, and in this condition it may be kept for an indefinite time.

To cause the image to appear instantaneously and in more than its pristine vigor, dip the paper in a weak solution of hyposulphite of soda; or, preferably, dip a piece of white blotting-paper in a solution of hyposulphite of soda and dry it. This prepared paper may be kept in contact with the latent picture so long as moisture is excluded. When it is required to develope the image, moisten the blotting-paper with common water and press it against the albumenized surface of the print, when, presto! the "magic photograph" is produced, and is, when washed, as permanent as many of the photographs of the present day. The image, by being again immersed in the bichloride of mercury solution, may be once more rendered invisible, and, by the hyposulphite solution, again restored as often as may be desired.

Although we believe that this process of magic photography has been patented a few weeks since, every intelligent photographer who has made himself acquainted with the past history of our art-science, knows that many years have elapsed since this "magic" process was first practised.

The amusement that can thus be introduced into the social circle by the "magic photographs" may be easily conceived.

New Process for Indigo Dyeing. By M. J. C. Leuchs.*

From the London Chemical News, No. 330.

Before it can be used for dyeing, indigo must be rendered soluble in alkaline and caustic solutions by being treated by a reducing body. By this reaction indigo loses its color, but after being fixed on stuff and exposed to the air it absorbs fresh oxygen and returns to its original color. This process, theoretically so simple, is practically complicated by serious difficulties, and requires, on the part of the dyer, much practice and great dexterity. Thus, for instance, with indigo reduced by fermentation with vegetable matters, in a caustic solution, the various acids produced during the fermentation combine with the alkali, the liquid soon ceases to be caustic, and loses the property of dissolving the reduced indigo. To remedy this a fresh quantity of alkali (soda, potash, or lime) must be added from time to time; but should an insufficient quantity be added, a portion of the reduced indigo remains undissolved, and soon decomposes under the fermenting matter. If, on the contrary, an excess of alkali be added, a certain quantity of white indigo is lost by its combining with potash, and forming an insoluble product.

According to M. Leuchs, of Nuremburg, all these objections are obviated by effecting the change from blue to white indigo by pectine. Pectine consists in considerable quantities in the turnips of different species, in pumpkins, melons, &c.; it may be extracted from these fruits, or they may even be directly used to reduce indigo. The most simple process consists in heating 45 or 50 kilogrammes of the caustic ley to 75° C., adding half a kilogramme of well pulverized indigo, then suspending in the vat a kind of basket of iron wire, containing from 8 to 10 kilogrammes of fresh turnips, cut into small pieces. Then heat gradually to boiling point; the indigo soon loses its color, and the solution decanted into special vats and diluted with water freed from air, will be ready for dyeing purposes. Contact with air must, of course, be as for as possible avoided.

When the dye-bath is exhausted it may serve for a fresh operation by adding indigo, a little caustic soda, and boiling it as above with a certain quantity of turnips.

On the iron wire trellis there will remain hardly 5 or 6 per cent. of the original quantity of turnips. This residue may be used in paper making.

The simplicity of this new process may easily be proved by introducing into a closed tube a small quantity of indigo mixed with a few drops of soda or caustic potash, adding a small piece of turnip, and boiling; the indigo will rapidly lose its color, and redissolve and return to its original color by exposure to the air.

As turnips are not everywhere cultivated, and during certain seasons are not to be procured fresh, the author has found that the active principles may be extracted by boiling the turnips with water, under

* *Bulletin de la Société Chimique*, February, page 153.

a pressure of two or three atmospheres. C. Leuchs & Co., of Nuremburg, now manufacture, on a considerable scale, an extract of turnips, 1 kilogramme of which will dissolve cold 4 kilogrammes of indigo.

Note on the Purification of Platinum. By Mr. E. SONSTADT.

From the London Chemical News, No. 330.

The tendency of platinum to alloy with other metals at a temperature far below its fusing point is sufficiently well known to every user of platinum crucibles. It is equally well known that iron, &c., which has been absorbed by platinum cannot be removed, except superficially, by the action of hydrochloric acid for instance, nor even by heating in acid sulphate of potassium. Stas, in his memoir on the atomic weight of silver, &c., states that he purified his platinum vessels from iron by causing them to come in contact, at a red heat, with the vapor of chloride of ammonium. The process had to be repeated as often as any yellow sublimate was formed. This process is less effectual, or less conveniently and speedily effectual, than the modification of it that I have to propose; because, if the vapor of the sal ammoniac is generated from the solid salt in the vessel to be purified, the heat absorbed in the vaporization of the salt tends to keep the vessel at a temperature below that at which volatile metallic chlorides are most readily formed. Instead of chloride of ammonium, I put dry double chloride of ammonium and magnesium in the platinum vessel intended for purification. The vessel is then heated to about the fusing point of cast iron for about an hour. I find a Gore's furnace convenient for this purpose. In this process, not only is chloride of ammonium vapor given off for a long while with the double salt, at a temperature much above that at which chloride of ammonium alone volatilizes, but when that salt is completely expelled, the chloride of magnesium remaining is perpetually being decomposed with evolution of free chlorine, and, frequently, the formation of a crystalline crust of periclase lining the crucible. Platinum thus purified is softer and whiter than ordinary commercial platinum. The method is not available solely for the removal of iron, but retrieves crucibles that have become dark colored and brittle from exposure to gas flame, as well as crucibles that have been attacked by silicates during fusion of these with carbonate of sodium. I cannot conclude this note without remarking on the extreme facility with which platinum becomes impure by heating in contact with matters containing only a very small proportion of substance capable of attacking the metal. Thus, a platinum crucible becomes sensibly impure after prolonged ignition at a high temperature, bedded in commercial magnesia. On the other hand, I have kept a platinum crucible at a constant weight to the tenth of a milligramme over a series of intense ignitions, when the precaution as been taken to bed it in chemically pure magnesia.

FRANKLIN INSTITUTE.

Proceedings of the Stated Monthly Meeting, May 16, 1866.

The meeting was called to order with the President, William Sellers, in the chair.

The minutes of the last meeting were read and approved.

The Board of Managers presented their minutes and reported, that, at their meeting on the 9th instant, they received donations to the library from the Royal Astronomical Society and the Society of Arts, London; l'Ecole des Mines, Paris, et la Société Industrielle de Mülhouse, France; der Oesterreichischen Ingenieur-vereins, und der Neideroesterreichischen Gewerbe-vereins, Wien, Austria; Isaac Newton, Esq., Commissioner of Agriculture, Frederick Emmerick, Esq., and George Metlar, Esq., Washington, D. C.; the Managers of the State Lunatic Asylum, Utica, and the Young Men's Association, Buffalo, New York; Edward F. Moody, Esq., Camden, New Jersey; and Henry Morris, Esq., Prof. John F. Frazer, the Guardians of the Poor, Dr. T. S. Kirkbride, and Robert S. Lewis, Esq., Philadelphia.

Three gentlemen were elected members of the Institute.

The various Standing Committees reported their minutes, and the Special Committee on Experiments in Steam Expansion reported progress.

The paper announced for the evening was then read by Mr. Emile Geyelin, on the Jonval Turbine, with special reference to the waterworks lately constructed at Montreal. The abstract of this paper will appear in our next number.

The Report of the Resident Secretary on novelties in Science and the Mechanic Arts was then read, as follows:

Secretary's Report.

Mechanics.—Improved Governor for Steam Engines, invented by David Shive of Philadelphia. In this instrument we have the following peculiarities of construction: The rods carrying the balls, in place of being so hung as to have a motion in a radial direction, as in the ordinary governor, are so arranged that their direction of motion is half way between a radial and tangential plane. This adjustment gives great delicacy or sensitiveness to any change in velocity, as is claimed, for the following reasons: Every change of velocity acts initially in a tangential direction upon the rods and balls. If, then, their only motion is in a radial direction, the first effect must be a binding or jamming at the guides and joints, succeeded by a radial motion, only as the resultant of the forward motion and this resistance. For this reason it might seem that a tangential motion would be the best to allow in the balls and arms, and such would be the case if the governor were normally at rest, and started back and forward when brought into action; but, being normally in motion, a resultant centrifugal tendency is developed, which would cause it to bind upon the

tangential guide or joint. These two difficulties are compromised, and the best result obtained, therefore, by an intermediate position of the plane of movement for the rods and balls. The nearly vertical direction, in which the balls hang, also adds to this same property of sensitiveness.

An Apparatus for the Preparation of Aromatic Infusions, invented by J. H. See of Charlestown, Mass., is constructed as follows: The lower portion, which may be detached from the rest by a single motion, is called the boiler, and consists of a closed cup or cylindrical vessel, having in its centre an open tube, running nearly to the bottom, around which several holes are pierced through the upper surface of the cup. These are covered by a loose rubber washer, which, however, is pressed firmly upon and closes them when the upper portion of the apparatus is attached in its place. This consists of a large vessel to receive the infusion when made, having a central tube, which, when in place, is continuous with that in the "boiler" already described. Upon the summit of this tube fits the central tube of a cup, with a finely perforated bottom, in which the substance to be treated, such as coffee, tea, &c., is placed. A lid to this cup, held down by friction and a spring within an outer lid, which covers all, acts as a safety valve to this apparatus. Water being placed in the boiler, and heated until it boils, the steam escapes under the loose washer; but when the upper part is secured in its place, this outlet is closed, and the steam, accumulating, forces the water to ascend the central pipe, to overflow into the cup, and, having percolated the matter there placed, to descend into the large receptacle. When the water has been thus driven out of the boiler, this may be separated from the rest of the apparatus. The water thus acting at a very high temperature, and under pressure, will remove most thoroughly all extractive matter from the substances treated.

The Rotary Steam Engine, invented by Matthew Fletcher, was described, as follows: A cylindrical drum, set eccentrically within a cylinder, is provided with four radial slots, in which work blades or flat-boards, which, as the drum revolves, are caused to move in and out.

Physics.—Freezing by Spray of Ether.—At a previous meeting the ingenious toy, called an "atomizer," by which perfumed liquids could be scattered through the air of an apartment, had been described and exhibited, when it was suggested that an action, capable of many useful applications in the future, was here represented. One of these applications, itself also but the forerunner of many more, was now before us, in the apparatus for production of intense cold and anæsthesia, invented by Dr. Richardson of England. By very simple mechanism, a constant blast of air was driven into a double nozzle, through the inner tube of which a jet of ether was forced from a bottle by part of the same air, while a larger portion escaped around the ether jet, scattering it in fine spray, and evaporating it, so as to produce very intense cold. The bulb of a thermometer, introduced into this spray, showed a temperature several degrees below zero, and the water in a

small glass test-tube was frozen in less than a minute when similarly placed. (This action was experimentally demonstrated.)

An interesting table, showing the temperatures obtained by the use of various volatile liquids with this apparatus, will be found in the *Chemical News* for this year, page 196. Pure ether gives the lowest —6° F; bisulphide of carbon, next, 0° F.; then aqua ammoniæ, specific gravity 88, 12·2° F.; and then alcohol, absolute, 17·6° F.

Freezing Machines Incorrectly Described.—In most of the foreign journals we find accounts of two machines, said to have been exhibited, which are so full of the grossest mistakes and impossible statements, that their insertion without comment implies, on the part of the respective editors, a free use of the useful scissors, unchecked by the labor of perusal.

In the first place (see *Practical Mechanics' Journal*, *Mechanics' Magazine*, &c., for December last) we have a machine in which, it is stated, that amylic ether (oxide of amyl) prepared from the peculiar alcohol of the potatoe, by treatment with sulphuric acid, is *liquefied by a pressure of 5 to 7 atmospheres*. We here remind our readers that this amylic ether is a *liquid boiling at* 348·8° F. The description quoted then proceeds to state that, after *becoming gas* and *forming ice*, this substance goes into spirals, where it freezes the water, and, after being *vaporized*, is absorbed by sulphuric acid. It is clear, of course, to all, that if the process described has any existence, methylic (not amylic) ether must be employed; but if we make this correction, we then find another mistake, since this body is not prepared from potatoe, but from wood spirit, and we still have the lamentable confusion of a double vaporization and duplicated freezing.

Again, we hear, in the *Chemical News*, page 118, and the *Mechanics' Magazine*, page 165, that "Signor Toselli operates a freezing machine by heating salt water to 100°, in a strong iron vessel, and collecting the vapor in another." The choice as to where the freezing occurs seems to be left to the fancy of the reader, which we also will not attempt to limit; for one place seems about as likely, *or unlikely*, as another.

Clay Gas Retorts, it is argued by a correspondent of the *Mechanics' Magazine*, are not, in the long run, economical, as compared with iron ones; for though they cost less and last longer, yet, on account of their inferior conducting power, they cause a waste of fuel, which more than compensates these other advantages. It is proposed, in this connexion, to use clay retorts next the fire, but iron ones in the upper part of each setting.

Artificial Stone, made by Ransome's process, from silicate of soda, sand, and chalk, seems to have been manufactured on a large scale, and pretty extensively employed, according to a long editorial notice in the London *Mechanics' Magazine*. The concentrated solution of silicate of soda is mixed with the other ingredients, when the whole is packed into moulds, from which the blocks are removed to a tank where they are basted, to use a culinary illustration, with a saturated solution of chloride of calcium, and are then subjected to a shower

bath, for the removal of soluble salts. By omitting the chalk, an excellent filtering stone is obtained. The artificial stone has been employed in the works of the Metropolitan Railway, on the London, Dover, and Cheltenham extensions, and elsewhere.

The Spectrum Band Peculiar to Sodium may be demonstrated to a large audience, by the yellow light given off by a rod of soda glass, when introduced in the oxohydrogen jet. This fact, first observed and demonstrated (as he believes) by Prof. Morton, was shown to the members present, as follows: An ordinary gas-microscope box, containing a lime light, has, in its front, a narrow vertical opening or slit, $\frac{3}{10}$-inch high by $\frac{1}{30}$-inch wide, distant $1\frac{1}{4}$ inch from the lime light. A common, uncorrected lens, of 7 inches focus, is so placed in front of this, as to give an image of the opening upon the screen. The whole apparatus then being turned away from the screen, one or two bisulphide of carbon prisms are placed in the path of the ray, and a spectrum, which may be readily made 15 feet long, and very bright, is thrown upon the screen. The lime cylinder or plate, which is giving the light, being then withdrawn, and a common glass rod substituted, the spectrum disappears, with the exception of a sharply defined yellow band, such as is produced by sodium in the spectroscope. This property of glass is curious, and probably results from the fact that this body, even when intensely heated, gives but a feeble light, while, where the jet strikes it, particles of sodium are instantly vaporized, and so intensely ignited as to produce a large volume of their peculiar yellow light. This light is clearly derived from the glass rod, and not from its more distant envelope of yellow flame, for if the rod is lowered out of the range with the opening, all light disappears from the screen, even though the yellow flame still reaches far beyond the opening in the lantern.

The Absorbent Power of Sodium Vapor or Yellow Light was then demonstrated by replacing the lime in the lantern, and then introducing a small Bunsen burner, in which a globule of sodium was supported by a platinum spoon, between the light and opening. In a few seconds the sodium was vaporized, and a black band was seen to cross the brilliant spectrum in the place before occupied by the yellow line given by the glass. The absorptive power of hyponitric acid, and of ammonia solution of cochineal, was also demonstrated. The arrangement of apparatus used on this occasion is of special interest, because it renders possible the demonstration to large audiences of the interesting phenomena of spectrum analysis, the absorption bands, &c., with the easily manageable lime light in place of the expensive and troublesome electric apparatus otherwise demanded.

Chemistry.—A new Process for the Manufacture of Oxygen is announced by M. Archereau. The substance employed is sulphuric acid, but the details are not given.

Chromate of Lead is apt to change from its first clear yellow to an undesirable orange tint. This may be prevented by keeping it for some time in the dark. It is also less likely to occur when nitrate of lead is used in the preparation than when the acetate is employed.

A new Form of Phosphorus, occurring as metallic crystalline plates and microscopic rhombohedra, is formed by dissolving red phosphorus in melted lead, and allowing it to crystallize out, on cooling. (See *Chemical News*, page 133.)

The Decomposition of Common Salt by Steam at very high temperatures, has been suggested by some observations on the products of volcanic action; but in the *Mechanics' Magazine*, page 197, we find a full discussion of the many attempts which have been already made in this direction, and of their ill-success.

A Test for Carbolic Acid, by which its usual adulteration with oil of tar may be readily recognized, is furnished in the solubility of the former in 25 to 70 parts of water, or in 2 parts of caustic soda solution. Oil of tar is nearly insoluble in both.

The Intermittent River, Bourne, making its appearance every 6 or 8 years, was, on the 13th of March, flowing past Croydon, England, at the rate of 20,000,000 to 30,000,000 of gallons per day.

The Analysis of the Red Sea Water lately made by MM. Robinet and Lefort, shows its identity with ordinary ocean water, and difference from that of the Dead Sea, thus disproving the supposed connexion of these by any subterraneous communication.

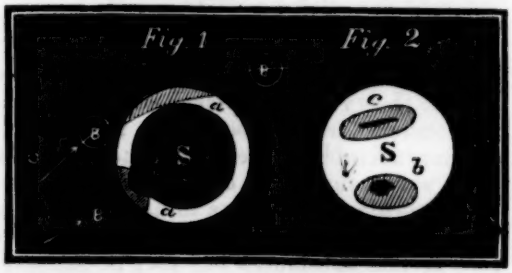
At the conclusion of the Secretary's Report the meeting was, on motion, adjourned.

HENRY MORTON, *Secretary.*

The following abstract of remarks made at our last meeting was received too late for insertion in our previous number:

Origin of Spots on the Sun's Disk. By JOHN W. NYSTROM, C. E.

The spots seen upon the solar disk are, in my opinion, occasioned by large bodies falling into and making orifices in the sun's luminous envelope or photosphere, thereby forming the spots, which may be explained by the figures on the black-board, as follows:



Let Fig. 1 represent a section of the sun, of which the shaded part S is its supposed solid and opaque body, with its surrounding luminous atmosphere or photosphere *a a*. A body B striking in the direction of the arrow perpendicular to the surface would make an orifice in the luminous atmosphere appearing as a round spot *b*, Fig. 2; but when the body strikes the sun's crust it will probably be broken to pieces, and reacting thus upon the so-called photosphere, make the hole or spot more or less irregular.

It would be a rare chance that the body should arrive in a straight line towards the centre of the sun, but would generally move in a parabola, or some conic section, with the sun's centre as the focus, and parameter of its course may be such as to pass the body B′ only through the luminous atmosphere, in which case the spot would appear oblong, as at *c*, Fig. 2. In most cases of bodies falling into the sun, the parameter of their course is such that the crust will be struck obliquely and the body burst to pieces, of which some fragments may glance off and fall back, and thus produce numerous other spots of various shapes and magnitudes.

Some striking bodies may be a great many thousands of miles in diameter, and make apertures which would require a time of many weeks or months to close up; regard being had to the peculiar constituency of the so-called sun's atmosphere.

Smaller bodies, which are probably very numerous and constantly falling into the sun, do not produce holes large enough to be visible through our present telescopes.

The appearance of the sides of the spots, or rather, the section of the luminous substance which envelops the body of the sun, leads us to suppose it to be a highly plastic or semi-fluid substance, which, by some physical action, is luminous only on its outer surface.

When a projectile strikes an armor-plate, the heat generated may be so intense as to actually melt some of the iron. When we know the weight and velocity of the projectile, or of the body striking the sun, we can calculate with great precision the quantity of heat generated in the collision.

The matter thus constantly flowing into the sun generates a great deal of heat in the collision besides that which is due to its own combustion. The sun is thus supported in its prodigal supplies of light and heat, which are the forms in which matter is returned throughout space, an operation which appears to be perpetual like that of rain and evaporation on the earth's surface.

There have been advanced two hypotheses regarding the physical constitution of light and heat, namely, the theory of *atomic emission* and the theory of *undulatory vibration*. The truth is, most probably, the combined result of both these theories, or that light and heat, &c., are imponderable matter propagated by undulatory vibrations, or, in other words, that a ray of light from the sun may be composed of all the elementary substances in nature, as indicated by the solar spectrum. The lines seen in spectrums of different lights not only represent certain substances but may be samples of the very atoms, and that each substance produces its own color and requires a specific grade of vibration for its propagation.

It appears that all matter is capable of being converted into four aggregate forms, namely, *solid*, *fluid*, *gaseous*, and *imponderable*. When it becomes *imponderable* it has, of course, no weight, because it is resolved into that very essence of matter, the action and reaction of whose atoms may constitute force or gravity.

The different forms of imponderable matter, such as *light*, *heat*, *ac-*

tinism, *electricity*, and *magnetism*, appear to be different compounds, which seem clearly to indicate their materiality, and that they possess inertia, of which the most striking illustration is the flow of electricity through Geissler's tubes.

The electric fluid flows through the irregularities of the tubes the same as a gas or a liquid would do; that is, its velocity through the narrow cylindrical parts of the tubes is continued by the inertia some distance into the globes. The bolt of lightning which tears asunder houses and trees, and ploughs up the ground, plainly indicates this to be the work of the inertia in the electric fluid.

The fact that a fine platina wire becomes red-hot when electricity passes through it from a thicker copper wire, proves that the imponderable fluid is squeezed into a narrower space with greater velocity than that which it passed through the copper wire. Such an operation cannot be explained simply by the undulatory law of vibration.

The very fact that light is refracted through a transparent body indicates light to be a material substance, and the fact that a ray of light is dispersed when passing through a prism to the spectrum, also indicates that it is composed of different substances, each of which takes its peculiar course of refraction.

The physical constitution of forces and of the undulatory vibration is yet a mystery to us. The undulatory vibration of the imponderable substances is evidently of two kinds and *in opposite directions*, constituting the forces of attraction and repulsion. The vibration of light emanating from the sun must evidently be opposite to the vibration forming the sun's attraction.

The intensity of light is analogous to temperature in heat, and can be converted into power or into the product of force, F, and velocity, V, whilst quantity of light is analogous to quantity or units of heat, which are convertible into physical work, or into the product of the three simple physical elements: force, F, velocity, V, and time, T.

It appears that F V T is a trinity of physical elements which governs the material universe. All action of whatever kind, whether mechanical, chemical, or derived from light, heat, electricity, or magnetism; all that has been or is to be done or undone, is comprehended and expressed by the triune function F V T. It is omnipotent, ubiquitous, and eternal.

COMMITTEE ON SCIENCE AND THE ARTS.

Report on a System of Protection against Lightning for the Blockley Alms-house.

The Committee on Science and the Arts constituted by the Franklin Institute of the State of Pennsylvania, for the promotion of the Mechanic Arts, to whom was referred for examination the subject of a System of Protection against Lightning for the Blockley Alms-house,

REPORT: That the principal buildings of the Blockley Alms-house are erected around a square or court which contains within it the en-

gine and boiler house; the lengths of the buildings being, respectively, five hundred and twenty (520) feet on the south-east and north-west fronts, and three hundred and twenty (320) feet upon each of the other sides. Each front consists of a centre building connected by wings with end pavilions. The centre buildings of the north-east and south-west parts are ninety-four (94) feet in width and ninety-eight (98) feet deep, fifty-seven (57) feet in height from the ground to the top of the cornices and sixty-eight (68) feet to the combings of the roof; the tops of the chimnies, which are almost midway between the eaves and the combing, rising about two and a half (2½) feet above the level of the comb. The roof of this building is tin. The wings are one hundred and forty-seven (147) feet long by sixty-two (62) feet in depth, fifty-one (51) feet four (4) inches in height to the cornice, and the tops of the chimnies are nine (9) feet six (6) inches above the cornice. The roofs are gravel. The end pavilions are sixty-two (62) feet long by sixty-four (64) feet deep; the height of cornice and chimnies the same as on the wings. The roofs are of tin. On the north-east and south-west fronts the corresponding parts have the same heights and the roofs are of the same materials, (except that the roof of the eastern pavilion is of iron, which, in an electrical point of view, is an immaterial difference.) The centre buildings on these fronts are sixty-two (62) feet long by eighty-three (83) feet deep, the wings are one hundred and sixty-five (165) feet by seventy (70) feet, and the pavilions fifty-eight (58) feet by one hundred and twenty-six (126) feet.

Around the buildings, on the inner side, at a distance of seventeen (17) feet from the inner line of the south-east and north-west buildings and of forty-five (45) feet from the others, runs a line of four and a half (4½) inch cast iron pipe connected with the heating apparatus, and thus communicating with every part of the buildings and with the engine house.

The elements of efficient protection against lightning are, sufficient conductors, elevated enough above the highest points of the buildings to be protected, and close enough together to leave no portion unguarded, terminated above with sharp and durable metallic points to exercise their inductive action upon the clouds, and connected below with a sufficient surface of water or constantly moist ground to ensure easy electrical discharge. These conductors should all be connected together and have all large masses of metal in the building or its immediate vicinity connected with them by metallic communication.

To apply these principles to the building before us, we recommend that rods of one and a half (1½) inch round iron be erected on the chimnies of each front, (upon that side of the chimney which is towards the combing of the roof,) and that these rods be raised at least fifteen (15) feet above the tops of the chimnies, and, if necessary, properly staid by iron rods from the roof. (Experience has shown that a sufficient conductor is capable of protecting a circle around it whose radius is from two and a half (2½) to three (3) times the height of the point of the conductor above the plane to be protected. The chimnies on the centre building of the Alms-house are from six (6) to seven (7) feet

above the roof and about twenty (20) feet apart. On the other building they are four and a half (4½) feet in height and fifty (50) feet apart; from which it will be seen that rods raised fifteen (15) feet above the tops of the chimnies will cover with their protective influence the whole area of the roof.) At the level of the roof all these rods upon each building should be connected together by an iron rod of the same diameter, (1½ inches,) and this rod should, by soldering, be put into metallic connexion with each metallic roof over which it passes. At a convenient place on each building an iron rod of the same diameter should be connected with this and continued downwards to the ground and there put into metallic connexion with the pipes of the heating apparatus which surround the interior court. (Perhaps it would be better to have two such connecting rods for each building, one at each end.) The metallic pipes which are used to conduct the rain-water from the roof to the ground should also be put into metallic communication with the iron pipes of the heating apparatus. As an additional security, it would be expedient to connect the rods with the water and gas pipes of the house, the connexions to be made by soldering.

At their upper ends the lightning-rods erected upon the chimnies should be terminated by a brush or collar of points surrounding a central one. The points of the collar should be turned outwards at an angle of about forty-five (45) degrees, or, what would be better, the collar should be made with double the usual number of points, (say twenty (20),) and every alternate one turned out so as to be horizontal, the intermediate ones being bent outwards at an angle of forty-five (45) degrees. The committee recommend that the points should be of wrought iron and carefully and heavily gilded, so as to withstand all atmospheric influences, the length of the point being about six (6) times the diameter of the rod, or about nine (9) inches. If there be any difficulty found in having the iron permanently gilded, the points should be made of copper of the same dimensions and thoroughly gilded. The points ought to be, from time to time, examined, to see that they have retained their sharpness and that the gilding is intact.

With a system of rods thus provided, the work being well done, the Committee believe that the buildings will be entirely out of danger of any damage from lightning.

The centre buildings which contain the engine and boilers are already sufficiently protected by a rod erected upon the chimney.

JOHN F. FRAZER,
JOHN C. CRESSON,
FAIRMAN ROGERS,
Sub-Committee.

(By order of the Committee.)

WILLIAM HAMILTON,
Actuary.

Philadelphia, April 19, 1866.

A Comparison of some of the Meteorological Phenomena of APRIL, 1866, *with those of* APRIL, 1865, *and of the same month for* FIFTEEN *years, at Philadelphia, Pa.* Barometer 60 feet above mean tide in the Delaware River. Latitude 39° 57½′ N.; Longitude 75° 11½′ W. from Greenwich. By PROF. J. A. KIRKPATRICK, of the Central High School.

	April, 1866.	April, 1865.	April, for 15 years.
Thermometer—Highest—degree,	82·00°	79·00°	88·00°
" date,	21st.	27th.	24th, '61.
Warmest day—mean,	73·17	68·83	74·39
" " date,	21st.	27th.	29th, '56.
Lowest—degree,	37·00	40·00	20·00
" date,	8th.	9th.	7th, '57.
Coldest day—mean,	38·67	45·50	27·70
" " date,	8th.	9th.	2d, '57.
Mean daily oscillation,	14·17	14·33	16·31
" " range,	6·09	5 99	6·21
Means at 7 A. M.,	50·30	51·25	46·16
" 2 P. M.,	61·28	62 03	57·73
" 9 P. M.,	54·80	54·48	49·94
" for the month,	55·46	55·92	51·28
Barometer—Highest—inches,	30·252 ins.	30·265 ins.	30·518 ins.
" date,	17th.	9th.	3d, '54.
Greatest mean daily press.	30·203	30·208	30·458
" " date,	17th.	9th.	3d, '54.
Lowest—inches,	28·820	29·404	28·820
" date,	23d.	1st.	23d, '66.
Least mean daily press.,	29·051	29·497	28·959
" " date,	23d.	1st.	21st, '52.
Mean daily range,	0·145	0·172	0·167
Means at 7 A. M.,	29·832	29·897	29·821
" 2 P. M.,	29·771	29·844	29·778
" 9 P. M.,	29·800	29·889	29 810
" for the month,	29·801	29·877	29·803
Force of Vapor—Greatest—inches,	0·605 in.	0·689 in.	0·689 in.
" date,	21st.	29th.	29th, '65.
Least—inches,	·094	·120	·066
" date,	9th.	24th.	13th, '52.
Means at 7 A. M.,	·266	·264	·234
" 2 P. M.,	·302	·303	·251
" 9 P. M.,	·305	·280	·254
" for the month,	·292	·282	·246
Relative Humidity—Greatest—per ct.,	90·0 per ct.	93 per ct.	100 per ct.
" date,	19th.	12th.	often.
Least—per ct.,	18·0	23·0	13·0
" date,	29th.	24th.	13th, '52.
Means at 7 A. M.,	69·2	67·0	71·0
" 2 P. M.,	51·9	52·6	51·4
" 9 P. M.,	67·1	64·4	67·3
" for the month	62·7	61·4	63·3
Clouds—Number of clear days,*	7	9	8·4
" cloudy days,	23	21	21·6
Means of sky cov'd at 7 A. M.,	67·3 per ct.	61·3 per ct.	63·0 per ct.
" " " 2 P. M.,	71·3	64·0	66·7
" " " 9 P. M.,	54·3	51·0	53·5
" " for the month	64·3	58·8	61·0
Rain—Amount,	2·922 ins.	2·829 ins.	4·687 ins.
No. of days on which rain fell,	10	11	12·8
Prevailing Winds—Times in 1000,	N74°45′W·141	S53° 8′ W·179	N66°52′W·149

* Sky one-third or less covered at the hours of observation.

INDEX.

Aboriginal Ingenuity.—The Tepiti..... 124
Adhesion of Locomotive Engines, and Certain Expedients for increasing that function..... 86, 169
Aerostation,—Scientific..... 388
Algonquin and Winooska,—Results of the Trials of the U. S. Steamers.... 202, 235
Alloying of Iron and Steel with Tungsten..... 54
Alms-house,—Report on a System of Protection against Lightning for the Blockley..... 423
Alvaro Reynoso's Cold Process for Making Sugar..... 209
Ammonia as a Means of Utilizing Heat Force,—On the Use of Liquefied..... 284
Anemometer,—New Differential..... 279
Armstrong, (Sir W. G.) On Chain-testing Machines..... 55
Aromatic Infusions,—Apparatus for the Preparation of..... 418
Artificial Stone,—Process for making..... 45, 356, 419
Artlett, (R. W.) On the Preparation of Oxygen..... 44
Atlantic Telegraph Scheme,—The new..... 347

Balloon Committee,—Report of the..... 111
Beams,—On the Deflexion of..... 363
———,—On the Strength of Timber..... 309
Beckton, (J. G.) On the Construction of Blast Furnaces and Manufacture of Pig Iron..... 37
Bell, (A. H.) Telemeters, or Instruments for Measuring Distances..... 304, 366
Bibliographical Notices—
Student's Practical Chemistry. By H. Morton and A. R. Leeds..... 70
Strength of Cast Iron Pillars. By J. B. Francis..... 70
Facts about Peat as a Fuel. By T. H. Leavitt..... 214
Photographic Mosaics. By H. C. Lea and E. R. Wilson..... 215
The Celebrated Theory of Parallels. By M. Ryan..... 215
Blast Furnaces and the Manufacture of Pig Iron,—On the Construction of..... 37
———— with Blasts of High Temperature,—Effect of Blowing..... 46
Blasting,—On Nitro-glycerine for..... 346
Blockley Alms-house,—Report on a System of Protection against Lightning for the..... 423
Boiler,—Report of Explosion of a Steam..... 49
Boilers,—On Incrustation of Steam..... 18, 349
———,—Method of Preventing the Incrustation of..... 347
Boller, (A. P.) On the Camber in Bridges..... 289
———— the Williamsport Suspension Bridge..... 217
Borax Lake in California..... 282
Bourdon Pressure Gauge,—On the..... 73, 219, 361
Bridge,—Large Iron Railway..... 235
———,—The Williamsport Suspension..... 217
Bridges,—On the Camber in..... 289
Bromine and Iodine,—A Delicate Test for..... 359
Bryson, (W.) On Strength of Cast Iron and Timber Pillars..... 312
Bunsen Battery,—Substitute for Nitric Acid in the..... 358

California,—The Borax Lake in..... 282
Calvert, (F. C.) On Chemistry Applied to the Arts..... 27, 117
———— On the Hydraulic Properties of Magnesian Limes..... 180
———— On Important Chemical Discoveries..... 195, 257, 333, 406
———— and Johnson. On the action of Sea-water on Metals..... 25
Camber in Bridges,—A. P. Boller on the..... 289

Caoutchouc,—On the Decay of Gutta Percha and 123, 190
Carbolic Acid,—A test for 421
Cattle Plague and Carbolic Acid 359
Cement,—Experiments on the Strength of 166
Centre of Station and Oblique Illumination on Signals in a Geodesic Survey 98
Chain-testing Machines 55
Chemical Discoveries,—On some of the Most Important 195, 257, 333, 406
Chemistry Applied to the Arts 27, 117
———. By H. Morton and A. R. Leeds,—Notice of the Student's Practical 70
Chromate of Lead 42
Clay,—To Preserve the Plasticity of 44
Climate of San Francisco 354
Cloez, (M. S.) Experiments and Observations on the the Oxidation of Vegetable Oils 187
Coal Mines,—Fire-damp in 251
Colors Obtained from Coal Tar 407
Condensation,—On Surface 60
Copper,—Photographing on 344
Cork Springs,—Notice of 355
Cornish Engines,—Report on 111
Cowper, (E. A.) On Blowing Blast Furnaces with Blasts of High Temperature 46
Crucibles for Melting Steel 359
Cylinders,—On Increasing the Resistance to Rupture of Hydrostatic 34

Dalton's Thermometer 45
Davidson, (G.) Sextant with Spirit-level Horizon 292
Day, (J. V.) On Increasing the Resistance to Rupture of Hydrostatic Cylinders 34
Density of Saturated Steam expressed by a New Formula,—On the Law of 76
Dingler, (O.) and F. Pfeiffer. On Surface Condensation 60
Dupre, (M. Athanase.) The Number of Molecules in a Cube of Water 354
Dyeing,—A new Process for Indigo 415
Dyer, (J. C.) On the Origin of Several Mechanical Inventions, and their Subsequent Application to Different Purposes 247

Edmonds, (T. R.) On the Law of Density of Saturated Steam 76
Electric Light for Lighthouses 185
Electricity,—Fire in a Cotton Mill from 342
Emery, (C. E.) The Giffard Injector 52
Engravings,—The Employment of Steel for Transferring 247
Etching on Glass with a Dead Surface 359
Ether,—Freezing by Spray of 418
Explosion at the London Gas-works at Nine Elms 114
——— of a Steam Boiler,—Report on the 49
Explosive Compound 59, 394

Fairbairn, (W.) Considerations on the Loss of the iron Steamship *London* 374
Fibrous Substances,—On the Tenacity of some 262
Fire-damp in Coal Mines 251
—— in a Cotton Mill from Electricity 342
——proof Materials and Construction 295
Fletcher, (J.) Improvements in Heavy Tools for General Engineering 100
Floating and Immersed Bodies,—On the Resistance of Water to 12
Flooring,—Phillips' Patent Girder and Fire-proof 228
Flow through Apertures of Solid Bodies 271
Force,—Apparatus for Illustrating 395
——, Heat, and Light,—Researches on 198
Francis, (J. B.) Fire in a Cotton Mill from Electricity 342
Freezing at a depth of Twenty-five Feet,—Water 327
——— by Spray of Ether 418
——— Machinery incorrectly Described 419
Furnace Slag,—On the Utilization of Blast 131
Furnaces and the Manufacture of Pig Iron,—On the Construction of Blast 37

Furnaces with a Blast of high Temperature,—Effect of blowing Blast 46

Franklin Institute—
Proceedings of Monthly Meetings 65, 135, 211, 282, 354, 417
Annual Report of Board of Managers for 1865 139
Standing Committees for 1866 214
Committee on Science and the Arts—
Report on a System of Protection against Lightning for the Blockley Alms-house 423
Galy-Cazalet, (M.) Process for Converting any Mass of Cast Iron into Steel... 279
Gas Diffusion,—Prof. Graham's Law of 111
—— Engine,—The Lenoir 175
—— Retorts,—Clay 419
——-works at Nine Elms,—The Explosion of the London 114
Gases dissolved in Cast Iron 48
—— in Mines,—Apparatus for the Detection of Explosive 329
Gauge,—On the Bourdon Pressure 73, 219
Giffard Injector,—The 52
Girder and Fire-proof Floor,—Phillips' Patent 228
Glass Surfaces,—On the Fracture of Polished 51
Governor for Steam Engines,—An improved 417
Graham, (Prof.) Law of Gas Diffusion 111
Grant, (J.) Experiments on the Strength of Cement 166
Graphotype,—Producing from Drawings Blocks for Surface Printing 188
Gravity,—A Body has no Weight while Falling Freely by 23
Greene, (D. M.) On the Construction of Screw Propellers 1
———— On the Steam and Exhaust Ports of Steam Cylinders 223
Gun,—On Perkins' Steam 250
Gunpowder,—Non-explosive 393
Gurlt, (Dr. A.) Naval and Hydrographical Telemetry 370
Gutta Percha and Caoutchouc,—On the Decay of 128, 190

Hawkins, (J. T.) On the Bourdon Pressure Gauge 73, 161
———— On Vacuum and the Indicator 229
Heat,—A Problem on the Conversion of Mechanical Power into 323
——, Force, and Light,—Researches on 198
Hitchcock, (De W. C.) Producing from Drawings Blocks for Surface Printing 188
Hooper, (W.) India Rubber as an Insulator for Telegraphic Conductors 181
Hydraulic Engineering,—S. McElroy's paper on 145
Hydrocarbons. By M. De Wilde 322
Hydrographical Telemetry,—Dr. A. Gurlt on Naval and 370
Hydrostatic Cylinders,—On Increasing the Resistance to Rupture of 34

Ice Machine,—A New 346
Incrustation of Steam Boilers 18, 349
————————,—Method of Preventing the 347
India Rubber as an Insulator for Telegraphic Conductors 181
Indicator,—J. T. Hawkins on Vacuum and the 229
Indigo Dyeing,—New Process for 415
Inertia of Surfaces,—D. V. Wood on the Moment of 91
Insects,—Force of 186
Iodine and Bromine,—A delicate Test for 359
Iron and Timber Pillars,—Strength of Cast 312
———— Steel with Tungsten,—Alloying of 54
—— Cased Sea-going Turret Vessel 153
——,—Construction of Blast Furnaces and Manufacture of Pig 37
—— from Rust—Thin Sheet Iron,—Protection of 412
——,—Gases dissolved in Cast 48
—— into Steel,—Process for Converting any Mass of Cast 279
—— Pillars. By J. B. Francis,—Notice of Strength of 70
—— Railway Bridge,—Notice of a Large 235
—— Ship-building Work,—Improvements in Heavy Tools for 100
—— Steamship *London*,—Considerations on the Loss of the 374
Irrigation of the Valley of Chimbote, Peru. By S. McElroy 145

Ivory,—Artificial 281

Jets of Water.—Experimental data arranged, and some practical hints. By S. W. Robinson 377
Joule, (Dr.) On Magnetic Declination 123

Lead,—Chromate of 420
Lemon, (J.) On the Ventilation of Sewers 9
Lenoir Gas Engine 175
Leuch, (M. J. C.) New Process in Indigo Dyeing 415
Leveling and Surveying by means of a Visual Angle and Rod 13
Lewis, (T. H.) On Fire-proof Materials and Construction 295
Light, Heat, and Force,—Researches on 198
Lighthouses,—Electric Light for 185
Lightning,—On Deaths by 187
————,—Report on a System of Protection for the Blockley Alms-house against 423
Locomotive Engines,—Expedients for Increasing the Adhesion of 86, 169
London,—Considerations on the Loss of the Iron Steamship 374

Magnesia,—On the Hydraulic Properties of 132
Magnesian Limes,—On the Hydraulic Properties of 180
Magnetic Declination. By Dr. Joule 123
Mayer, (J. R.) Mode of Correction for the Reduction to Centre of Station, and for Illumination Signals in a Geodesic Survey 98
McElroy, (S.) Papers on Hydraulic Engineering 145
Measuring Distances,—Telemeters, or Instruments for 304, 366
Mechanical Inventions,—Notes on the Origin of Several 247
———— Power into Heat,—Problem on the Conversion of 323
Mercury,—On the Specific Gravity of 353
Metals,—Action of Sea-water on 25
Meteorologic Observations in Philadelphia 71, 141, 216, 287, 360, 427
Miller, (W.) On the decay of Gutta Percha and Caoutchouc 128
Mines,—Apparatus for the Detection of Explosive Gases in 329
Mirrors Plated with Platinum,—Notice of Glass 357
Molecules contained in a Cube of Water,—The Number of 354
Moment of Inertia of Surfaces,—Prof. D. V. Wood on 91

Naval and Hydrographical Telemetry 370
Newton, (I.) Expansion Trials with U. S. Steamers Algonquin and Winooski 235
Nichol's Prism,—A New Form of 357
Nitric Acid in the Bunsen Battery,—Substitute for 358
Nitro-glycerine for Blasting 346
Nystrom, (J. W.) On the Origin of the Spots on the Sun's Disk 421

Oar and Paddle,—On the 154, 234
Ocean and Sea Temperature,—Specific Gravity of the 253
Oils,—Experiments and Observations on the Oxidation of Vegetable 187
Oxygen,—Preparation of 44

Papworth, (W.) On the Strength of Timber Beams 309
Parallels,—Notice of the Celebrated Theory of 215
Parkes, (A.) On the Properties of Parkesine, and its Application to the Arts and Manufactures 264, 384
Parkesine, and its application to the Arts and Manufactures 264, 384
Peat as a Fuel,—Notice of Facts about 214
Pendred, (V.) On Increasing the Adhesion of Locomotive Engines 86, 169
Perkin's Steam Gun 250
Phillips' Patent Girder and Fire-proof Flooring 228
Phosphorus,—A new Form of 421
Photographic Mosaic,—Notice of 215
Photographing on Copper 344
Photographs on Paper in their Natural Colors 413
Photography,—Magic 414
————,—Improvements in 196

Pillars,—Notice of J. B. Francis on Strength of Cast Iron 70
——,—W. Bryson on the Strength of Cast Iron and Timber 312
Plants,—Number of Useful 110
Platinum,—Glass Mirrors Plated with 357
——,—Notes on the Purification of 416
Pressure Gauge,—The Bourdon 73, 219
Prism,—A New Form of Nichol's 357
Propeller of Uniform Expanding Pitch,—On the Construction of the Screw... 309
Propelling Blades,—Increasing the Speed of Steamers,—Form of 154, 234

Railway Bridge,—Large Iron 235
Rankine, (W. J. M.) On the Tenacity of some Fibrous Substances 262
Ransome's Process for Making Artificial Stone 419
Red Sea Water,—Analysis of the 421
River Bourne,—The Intermittent 421
Robinson, (S. W.) Jets of Water—Experimental data arranged, and some practical hints. 377
—— Leveling and Surveying by means of a Visual Angle and Rod 13
Rolland, (M. Eug.) Steam Pressure Regulator 322
Rotary Steam Engine,—Notice of a 418

Salt by Setam,—The Decomposition of Common 421
San Francisco,—The Climate of 354
Scientific Aerostation 388
Screw Propeller of Uniform Expanding Pitch,—On the Construction of the 1
Sealing Wax,—The Cohesive Force of Red 264
Sea-water on Metals,—Action of 25
Sewers,—J. Lemon on the Ventilation of 9
——,—On the Effluvia from 50
Sextant with Attached Spirit-level Horizon 292
Simonin, (M.) On the Climate of San Francisco 354
Slade, (F. J.) The Lenoir Gas Engine 175
Slag,—On the Utilization of Blast Furnace 331
Sodium Vapor or Yellow Light,—The Absorbent Power of 420
Solid Bodies,—On the Flow through Apertures of 271
Sonstadt, (E.) Notice of the Purification of Platinum 416
Specific Gravity of Mercury 353
—— of the Ocean and Sea Temperatures 253
Spectrum Band peculiar to Sodium 420
Spots on the Sun's Disk,—J. W. Nystrom on the Origin of the 421
Springs,—Notice of Cork 355
Sprengel, (H.) Researches on the Vacuum 396
Steam and Exhaust Ports of Steam Cylinders. By D. M. Greene. 223
—— Boilers,—Incrustations of 18
——,—Report of Explosion of 49
—— Engine,—Notice of a Rotary 418
—— Engines,—An improved Governor for 417
—— Expressed by a New Formula,—The Law of Density of Saturated 76
—— Gun,—On Perkins' 250
—— Machinery,—Accidents from 50
—— Pressure Regulator of M. Eug. Rolland 322
——ship *London*,—Considerations on the Loss of the 374
Steamers,—On Increasing the Speed of 154, 234
—— Winooski and Algonquin,—Results of the Trials of the U. S. 202, 235
Ste. Claire-Deville, (M. H.) On the Hydraulic Properies of Magnesia. 132
Steel,—Crucibles for Melting 359
—— for Transfering Engravings,—On the Employment of 247
——,—New Process for Converting any Mass of Cast Iron into 279
—— with Tungsten,—Alloying Iron and 54
Stenhouse, (Dr. J.) Water-proofing Vegetable and Animal Tissues and Fabrics 406
Stewart, (B.) On the Specific Gravity of Mercury 353
Stone,—Process for Making Artificial 45, 356
——,—Ransome's Process for Making Artificial 419
Sub-marine Telegraphic Lines,—Retardation of Electric Waves in 179